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OF THE

SOCIETY OF TELEGRAPH ENGINEERS AND OF ELECTRICIANS,

INCLUDING

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* The Historical Exhibits of the Postal Telegraph Department are in the Central Pavilion, together with their Modern Exhibits.

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Telegraph Engineers and of Electricians.

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The Ninety-fourth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Wednesday evening, January 26th, 1881—Mr. W. H. Preece (President) in the Chair.

The minutes of the Ordinary General Meeting, and the Annual Meeting held on December 22nd, were read, and the names of new candidates announced.

Mr. W. H. Preece: Gentlemen,—The time has arrived when I must throw off that imaginary mantle which I have held for the past twelve months as your President, and throw it over the shoulders of this more worthy representative, Professor G. C. Foster, F.R.S., whom I have now great pleasure in introducing to you as your President for the ensuing year.

Sir C. F. Bright: The pleasant duty has fallen on me to-night of proposing the thanks of this Society to Mr. W. H. Preece for the admirable manner in which he has discharged the duties of President during the past year. I am quite sure that all of you will support the proposition most heartily. It is no easy matter to be President over a Society such as this, which has so great a variety of subjects coming before it, and which require to be introduced and dealt with more or less by the President. Still more arduous must the post be when he who undertakes such functions

is at the same time engaged, as Mr. Preece has been, in the performance of the many and varied duties of the official position held by him in the Post Office Telegraphs. I have known Mr. Preece for nearly thirty years. We were fellow-workers in the early days of that telegraph world in which he has so much distinguished himself. Not only has he done so well for this Society, but he is a man of rare inventive genius, as is shown by his improvements in railway telegraph signalling and many other things which are well known to all readers of electrical works, especially those treating of the technical details of telegraphs. I most heartily congratulate my friend on the admirable manner in which he has presided over us while holding the helm of this Society during the past twelve months. We have had during that period some most important papers. He has been a good caterer to our mess, introducing to us the best dishes and the newest in the season, and I am confident that the year 1880 will be marked in the history of this Society as one which can hardly be surpassed in its future history. Very appropriately, just now that we have changed the title, and have added the name "Electricians" to the old title of the Society, we have in Mr. Preece's successor a man of high distinction and genius in physics and natural philosophy. I beg to propose (and I am sure it will be carried with acclamation) that the best thanks of this Society are due to Mr. Preece for the admirable manner in which he has discharged the duties of President during the past year.

Professor W. G. Adams: I have great pleasure in seconding the vote of thanks which has been proposed by Sir C. Bright. It is unnecessary that I should take up much of your time in doing so, for you all know the way in which Mr. Preece has exerted himself during the past two years to make our meetings interesting, and a very little reflection will bring to mind the very interesting meetings we have had during that time. I need say no more. I have very great pleasure in seconding the proposition.

Professor G. C. Foster (President): It affords me great pleasure that my first official duty is to put this vote of thanks for your late President. I most heartily join in the proposition, and can only hope that you may not too greatly regret the proceedings of the Council in electing me to succeed Mr. Preece, but I shall do

my best to prevent the contrast between your late and new President being too keenly felt.

Mr. W. H. Preece: Mr. President and Gentlemen,—I had placed in my hands not many days ago a paper (I think from the other side of the Atlantic) written by a member of the Diplomatic Service, which was headed "Butter with a Soapstone Basis." Now I do not want you to infer that any of the remarks made by Sir C. Bright are in any way characterised by butter, nor that the remarks made by Professor Adams had anything in them of the character of soapstone, but I am sure that Sir C. Bright introduced a little modicum of soapstone and Professor Adams a small modicum of butter. I do not think the encomiums they have passed upon me have been deserved. All that I can say is that during my term of office I have endeavoured to do my duty, and if I could not command success, at least I have tried to deserve it. I have sought your approbation by bringing before you every recent novelty in electrical science, by pressing and pestering all my friends to give you papers, and by doing my very utmost to make your evenings entertaining as well as instructive. At the commencement I pointed out how essential it was for the Telegraph Engineer to combine theory with practice, and to endeavour, as much as he possibly could, to further the interests of his profession by interlarding, as much as he could, theory with practice. I have done so in the papers that have been put before you during my term of office. We have, in that period, had some admirable papers, thanks to Professor Hughes, Professor Bell, Mr. Stroh, Dr. Siemens, Mr. Alexander Siemens, Mr. E. Graves, and others—papers containing a mixture of points practical and theoretical; and I am happy to think that the interest of our meetings has increased at a compound rate rather than diminished, for we all remember what a successful and interesting meeting signalised the last evening.

There are other matters that we have to congratulate ourselves upon, and not the least among them is that, thanks to the exertions of our honorary Treasurer (Mr. E. Graves), the Society has been brought into a position in which it is not only entirely out of debt, but with a balance to its credit; and more than that, we com-

mence this year with (and it gives me great pleasure to hand over to Professor Foster) a balance also of papers, which are already in our possession, to be read at the forthcoming meetings, and which I am sure will prove interesting.

I thank you all for the great assistance which I have received from every Member and Associate during the past year, and can only hope that Professor Foster will meet with as much kindness and attention at the hands of those who prepare and read papers as I have had. (Applause.)

INAUGURAL ADDRESS.

By G. Carey Foster, F.R.S., President.

When one finds oneself in a position of so much distinction as that to which I have been called by the Society of Telegraph Engineers and of Electricians, it is only natural to ask oneself the question, What can people mean by selecting me for such an honour? If the only reason for putting this question were the interest that the asker takes in all that concerns the person to whom it is addressed, there would be no excuse for mentioning it here. It seems to me, however, that it is a question which it is not only natural but very desirable that I should ask myself. For, clearly, the likelihood of my being able in any degree to carry out the wishes and intentions of the Society must greatly depend upon my forming a correct judgment as to what those wishes and intentions are.

I make no excuse, therefore, for having tried to make out what may have been the possible reasons that determined your last choice of a President. Most obvious among such reasons appeared simple and unearned kindness, for which I can only offer sincere and grateful thanks. But I think I cannot be wrong in supposing that another motive which contributed to the election of one who is no way connected with telegraphy or any other practical application of electricity, was the feeling that the Society of Telegraph Engineers is not, and does not wish to be, simply a professional society, but that it is as much concerned with the scientific principles which underlie the practical operations in which many of

its members are engaged, as with the details of these operations themselves.

Now, whatever view I may take of the wisdom of your recent choice, considered as a way of putting forward the scientific interests of the Society, I have ventured to take as the subject of what I have to say this evening, some considerations which have been suggested to me by the supposition that this was the meaning of that choice. My remarks will accordingly relate to some of the ways in which the scientific study of electrical phenomena and the application to practical purposes of the results of that study mutually affect each other.

It is a trite as well as true saying that practical inventions spring out of scientific discoveries. The chief practical inventions that we have to do with in this Society owe their origin to a burst of discovery which took place just over sixty years ago. It was on the 21st July, 1820, that Oersted made known the fact of the deflection of the magnetic needle by the galvanic current, and in less than twelve months from that date Ampère had demonstrated the reciprocal action of magnets on currents; he had also discovered the force exerted by currents upon each other; and he and Arago had discovered the production of temporary electro-magnets when a current is carried by means of a coiled conductor round a soft iron core. Scarcely had electric telegraphy thus become possible, when in 1821 Sir Humphry Davy produced a voltaic arc 1 inch in length between carbon poles connected with a battery of 2,000 cells, thus giving to his own generation a prophetic glance at what was to be the light of their grandchildren. Ten years later, in 1831, Davy's great pupil followed with the discovery of induced currents—a discovery of which it is hard to say whether the scientific or the practical results have been the most important.

Its fundamental character must have been evident from the first, not only in the number and variety of the phenomena wherein the principle of induction manifested itself, but perhaps even more in the mavellously beautiful relation of reciprocity in which these phenomena stood to those of electro-magnetism and electro-dynamics. Faraday's discovery was the complement of those

of Oersted and Ampère. Taken in conjunction with each other, the phenomena of electro-magnetism and those of induced currents are seen to be different aspects of the same facts. When thus known from both sides, these facts fall naturally into relation with the other systematised physical facts under the grand generalisation of the doctrine of Energy; whereas, were but one aspect of them known, the phenomena would belong to the domain of Natural History rather than to that of Scientific Physics.

On the other hand, the practical consequences of the discovery of induced currents have shown themselves year by year to be growing more and more rapidly both in number and importance. In illustration of this statement, I need not do more than remind the Society that it is by the help of induced currents that the electric light has ceased to be merely a "marvel of science," and has become a matter of interest to municipal corporations and limited liability companies; that electro-magnetic engines, instead of being things which would sometimes go round upon a scientific lecture-table, and sometimes would not, have become the means whereby water-power can be substituted for steam-power upon a practical scale; and that Graham Bell has made it easier to talk to a friend in the next county than it used to be if he were in the next room.

With this discovery of Faraday's we may say that the essential foundations of all practical applications of electricity made up to the present time had been completely laid. The rearing of the superstructure has depended on the progress of practical invention. So far as it has been aided by scientific enquiries, these have related to the more complete elucidation of the laws of known phenomena rather than to the discovery of new phenomena; or, in cases where they have resulted in new discoveries, the things discovered have been matters of more or less important detail rather than fundamental principles.

Always, however, when we find scientific discoveries giving rise to practical inventions, we may also find these inventions contributing to the advancement of science. If it is true that in many cases practice has been rendered possible by science, it is no less true that every scientific discovery which is applied for practical ends becomes more fertile, even of purely scientific results,

than it would have been had it not been so applied. If the outcome of the application is a new instrument, such as a magneto-electric machine, a telephone, or a mirror-galvanometer, this immediately becomes a new tool in the hands of the scientific investigator. If the outcome is a new process like electric telegraphy, it gives occasion for the observation of phenomena—say, electrostatic induction in galvanic conductors, or earth currents—which would otherwise, in all probability, have long remained unknown. It is of some aspects of this beneficial influence exerted by the progress of practical inventions upon the advancement of science that I wish chiefly to speak in what follows.

And there is another way in which practice reacts beneficially upon science, which is not less important than supplying it with new tools of research, or new phenomena for investigation—that is, in often shedding new light upon the relative importance of different branches of enquiry, and demanding an answer to questions which might otherwise have had long to wait for full attention.

When an operation is transferred from the laboratory to the manufactory, the conditions under which it is carried out are completely changed. It is not simply a change of scale, tons taking the place of grammes, and miles of centimetres: the change goes much deeper than this, and affects both the object in view and the criterion of success. The object of the scientific investigator is to get to *know* something, and his criterion of success is the fulness and definiteness of the knowledge he gains. The object of the engineer or the manufacturer is to *do* or to *make* something, and his criterion of success is that he is able to do or make this thing under certain conditions, and especially that he can do it or make it at not too great a cost.

It does not, however, follow from this that the knowledge which will enable a man to succeed in the application of a scientific discovery to practical purposes is of inferior quality to that of the professed man of science. It may be narrower in range, but in what relates directly to the particular subject he takes in hand, the practical man finds himself from the outset obliged to know matters which, when the only motive for enquiry is the simple love of knowledge, are commonly not investigated until a high stage of

development has been reached. In the ordinary course of scientific discovery, what may be called the qualitative aspects of phenomena are the first to attract attention. Quantitative knowledge comes later, and by degrees: first it is ascertained what are the conditions which cause an increase or decrease of the effects observed; next, numerical relations of an empirical kind are established between the variations of these conditions and the resulting variations of the effects. It is not until a phenomenon is sufficiently well known for definite mathematical expression to be given to its laws that what are called "absolute" values of the constants entering into the expression are required for scientific purposes, or that methods for the determination of such values can be devised.

On the other hand, when an operation has to be carried on, or some definite result is to be produced as part of a commercial undertaking, it becomes of the utmost importance from the very first that the effect of every essential condition should be known in such a way that it can be stated by reference to universally recognised standards.

The difference between the requirements of the two cases becomes very evident on considering an example of each kind. During the last hundred years some unknown number of thousands of electrical machines have been made and used for purposes more or less scientific, but after all this experience, who could draw up a specification for an electrical machine which should, with a stated number of revolutions, produce a known quantity of electricity—that is, which should charge a condenser of known capacity to a given potential? Passing from electrical machines to galvanic batteries, we pass from instruments whose only important uses are for scientific purposes to instruments of which the practical applications are far more widespread, if they are not intrinsically of greater importance, then their scientific uses. Accordingly, we find that the knowledge which we possess of what a galvanic battery can do is far more definite than what we have in relation to electrical machines. We can tell not only how much stronger or weaker the current produced by one battery will be than that produced by another battery in a circuit of equal

resistance, but, when the resistance is given, we can say how strong the current will be in each case,—how much silver it would deposit in an hour, how much heat it would evolve in a given piece of platinum wire, or what would be the strength of the magnetic field produced when it traversed a coil of known dimensions. And who can doubt that, if the practical uses of the electrical machine were comparable with those of the galvanic battery, our knowledge of its efficiency would soon be equally definite?

The necessity for practical purposes of having numerical data expressed in terms of generally accepted standards has exerted a beneficial influence of a very important kind on the progress of more than one branch of physics; but as our business here is with electricity, we need not go beyond the bounds of this science in endeavouring to trace such effects. The progress of electrical science within, say, the last thirty years, has been such as to amount almost to a transformation. Multitudes of new and important facts have been brought to light, but the effect due to the changed point of view from which the old facts are regarded has been greater than that of any newly-discovered phenomena. Matters that used to be the secrets of the high-priests—not because they were anxious to conceal them, but because they found no one to listen—are known to the multitude, and are to be found written in every tolerable text-book. The introduction of methods of absolute measurement has rendered possible the definite application to electrical phenomena of the doctrine of Energy, whereby their relations to each other and to other physical phenomena have been placed in altogether a new light. The share which the practical applications of electricity have had in accelerating this development of the mathematical theory of the science is so considerable, that it seems fitting to consider a little more in detail what the system of so-called "absolute" measurement implies in its application to electricity, and to note some of the most important matters connected with the development of the system.

Strictly speaking, to measure a thing of any kind is to ascertain the numerical relation between it and some magnitude of its own kind taken as a standard for comparison. In so far, then, as

absolute means that which is not *relative*, it is a contradiction in terms to talk about absolute measurements. But in physics, words are not always used in exactly the same sense as in metaphysics, and any physical quantity is said to be known in absolute measure when its magnitude can be expressed without reference to any other standard units than those of length, time, and mass. These are of course the units employed in dynamics in all statements involving the numerical values of forces or of the work done by them; and by adopting for the measurement of electrical magnitudes methods founded upon the intensity of the forces to which they give rise, or upon the amount of work done, mathematical physicists have been able to show how every such magnitude can be expressed in terms of the dynamical units. It is precisely the general adoption of a system of measurement founded on these principles that constitutes the most characteristic distinction between the electrical science of the present and that of a generation ago; and the history of its development affords a striking example of the manner in which the most abstract scientific investigations may indirectly render services of the greatest value to industrial pursuits.

Before methods of measurement can be devised, it is evident that clear conceptions must be formed of the things to be measured. Such conceptions usually grow up by degrees in many minds from indistinct beginnings, until, in some one mind, they take definite shape and receive the precise expression which makes it possible for them to become the subject of mathematical reasoning. For the first great step of this kind in connection with the subjects that most concern us here, we are indebted to G. S. Ohm, who in his memorable work on the Mathematical Theory of the Galvanic Circuit, published in 1826, first placed in a clear light the nature and mutual relations of the magnitudes we now speak of as electro-motive force, strength of current, and resistance. But the system of absolute measurements in electricity owes its origin to the celebrated paper published by Gauss in 1832,—*Intensitas vis magneticæ terrestris in mensuram absolutam revocata*,—in which he showed how the intensity of the earth's magnetic force and the magnetic moments of artificial magnets could be measured in absolute units.

In 1840, Weber showed that this was all that was required in order to furnish a method for the absolute measurement of *electric currents*, since, according to Oersted, there is magnetic force in the neighbourhood of a current. In the same year that Oersted made known his great discovery, Biot and Savart made a series of experiments by which they ascertained how the direction and intensity of the force exerted by a current upon a magnetic pole varied with the distance and direction of each part of the conductor relatively to the pole. Combining these results with those of Gauss, Weber defined the absolute unit of current-strength in terms which were equivalent to saying that, when a current is carried once round the circumference of a circle of unit radius, the numerical value of the strength of the current was equal to the numerical value of the magnetic force produced by the current at the centre of the circle, divided by the number of units of length in the circumference of the circle. In order to carry out practically the measurement of currents in this way, Weber devised the instrument which is still in most cases the best and most convenient for the purpose, namely, the tangent galvanometer.*

The first application that he made of this method of measurement was to determine the value of the so-called "electro-chemical equivalent" of water, that is to say, the quantity of water which is decomposed per second in a voltameter by a current of unit strength. The result obtained may be stated in the language of modern electricians, by saying that a current of the strength produced when an electro-motive force of 1 volt acts in a circuit of 1 ohm resistance would decompose 1 gramme of water in 2 *hours*, $57\frac{3}{4}$ *minutes*. The special importance of this result arises from the fact that, according to the law of electrolysis discovered by Faraday, the quantity of electricity needed to decompose a

* As a matter of historical accuracy, it should be mentioned that a tangent galvanometer had been constructed by Pouillet in 1837, that is, three years before Weber's instrument, and that, earlier still, in 1824, De la Rive had constructed a sine-galvanometer. But as neither of these physicists took account of the absolute value of the earth's magnetic force, their measurements of current strength were only comparative.

gramme of water is also the quantity needed to decompose a chemically equivalent quantity of any other substance. It thus enables us to calculate the quantity of material consumed in each cell of a battery of given construction when it maintains a current of known strength for a known time.

Weber's measurement has since been repeated with closely accordant results by Joule, Bunsen, Casselman, F. Kohlrausch, and others.

In 1851, Weber pointed out that a knowledge of the absolute intensity of the earth's magnetic force also furnishes a basis for an absolute method of measuring *electro-motive forces;* since, according to the laws of induced currents established by Faraday, whenever a conducting circuit is displaced in a magnetic field, an electro-motive force acts in it, the magnitude of which depends only on the dimensions and motion of the circuit, and on the strength of the magnetic field. It is evident that electro-motive forces, thus stated in terms of absolute magnetic intensity, would be known in absolute measure, for the dimensions and motion of the conducting circuit involve only the units of length and time.

In Ohm's theory of the galvanic circuit, the resistance of a conductor appears as the ratio of the electro-motive force, acting in it to the strength of the current which it produces. Accordingly, absolute units of current-strength and electro-motive force, derived from electro-magnetic effects in the way indicated, having been adopted, the definition of the absolute unit of *resistance* followed as a natural consequence—being the resistance of a conductor in which the unit electro-motive force produces the unit current. Weber had thus established theoretically a coherent system for the measurement of all the three fundamental constants of the galvanic circuit. Practically, it is sufficient that we should have direct methods of determining only two of these, since the mutual relations of any two determine the third. We have seen how Weber in 1840 gave an experimental method for the absolute measurement of currents, and in 1851 he made known two different methods which he had applied for the absolute measure of resistance. The results of these memorable experiments were embodied by Weber in three standard wires, which were deposited for future reference in the

Physical Institute of the University of Leipzig, after the absolute resistance of each had been determined. These resistances were, approximately, what would now be expressed as 10, 5, and 2·5 ohms respectively.

The importance of thus establishing a system of measurement for electrical quantities which should be independent of the dimension or construction of any particular instrument, or even of the special properties of any particular substance, was, with characteristic penetration, immediately recognised by Sir William Thomson. Sir William at once adopted and extended Weber's system. By a course of reasoning which consists essentially in equating the work done, when an induced current of constant strength is maintained by the motion of a conductor in a magnetic field, with the expenditure of chemical energy needed to keep up an equal current in the same circuit by means of a galvanic battery, he showed how the electro-motive force of a battery may be calculated from the thermal value of the chemical action which takes place in it when a current is passing. In this way, employing experimental data supplied respectively by Dr. Joule and by Dr. Andrews, he estimated the electro-motive force of a Daniell's cell to be what would now be called

1·074 volt according to Joule's numbers, and
1·101 volt according to those of Andrews.

These results were arrived at from consideration of the energy of the current, and did not depend on measurements either of current or of resistance; it is therefore interesting to compare them with those derived from measurements of these two quantities. The first recorded determination of the latter kind is due to Bosscha, who in 1856 found for the electro-motive force of a Daniell's cell a number equivalent to

1·026 volt.

In 1868, von Waltenhoven obtained a value equivalent to

1·094 volt.

Since resistance-coils of known absolute value have been in the hands of every electrician, measurements of the absolute electro-motive force of Daniell's, Grove's, and various other batteries,

have been made by several observers—notably by Professor F. Kohlrausch and by Mr. Latimer Clark.

The system of measures of which I have been speaking was established by Weber as a matter of purely scientific interest, but the development of electric telegraphy which had been going on meanwhile, and especially the extension of submarine telegraphy, had made methods of accurate electrical measurement matters of great practical as well as scientific importance. The consequence was that, by about 1860, those who were reponsible for the proper fulfilment of the electrical conditions essential to the success of the great enterprises that were by this time becoming common, not only felt the necessity of having exact experimental methods, but also in many cases recognised the importance of being able to express their experimental results in terms of a coherent system of standard units, so that measurements of one kind could be compared with measurements of related quantities without the introduction of unnecessary reducing factors. At the meeting of the British Association held in Manchester in 1861, a remarkable paper, in which the importance of these considerations was forcibly brought out, was read before the Physical Section by Sir Charles Bright and Mr. Latimer Clark; and at the same meeting the following resolution was passed by the General Committee: "That Professor Williamson, Professor W. Thomson, Professor Miller (of Cambridge), Dr. Matthiessen, and Mr. F. Jenkin be a Committee to report upon Standards of Electrical Resistance; and that the sum of £50 be placed at their disposal for the purpose." This was the afterwards well-known Committee on Electrical Standards, which, with various additions to its list of members, continued in existence till 1870, and in furtherance of whose labours sums amounting altogether to £590 were paid by the Association. I need scarcely remind the Society that it is to this Committee—and especially to three of its members, the late Professor Clerk Maxwell, Professor Jenkin, and Mr. Hockin—that we are indebted for "the ohm" as a generally accessible standard making a close approach to the absolute resistance of

10 million metres per second.

It would be tedious to attempt to indicate the ways in which

both the diffusion and the advancement of sound knowledge of electrical science have been promoted by the multiplication of carefully-adjusted standards of known absolute resistance, in the form of resistance-coils adjusted either by reference to the ohm or to Dr. Werner Siemens's mercurial standard. It may suffice to say that, while providing for their own needs by the construction of graduated series of resistance-coils, practical electricians have placed an invaluable instrument of research in the hands of scientific men, which they have not failed to appreciate and profit by.

From an experimental point of view standards of resistance are of greater importance than electrical standards of any other kind, on account of the great number of methods of practical measurement of other quantities which can be founded upon their use. It is therefore a matter for much satisfaction that, since the introduction of the ohm as a standard of resistance, several investigations should have been made, having for their object to determine the absolute value of the mercury-unit, or to ascertain how nearly the resistance of the ohm itself agrees with its intended value of ten million metres per second. The results of such investigations hitherto made known are given in the following table :—

Date.	Observer.	Resistance in $\frac{10^9 \text{ Centimetres}}{\text{Second}}$	
		Ohm.	Siemens's Mercury Unit.
1870	F. Kohlrausch ...	1·0196	0·9717
1873	Lorenz	·9797	·9337
1876	Rowland	·9912	·9446
1877	H. F. Weber ...	1·002	·9550

In this comparison 1 ohm is taken as being equal to 1·0493 mercury-unit, the ratio given by Hermann Siemens and by Kohlrausch. Kohlrausch, Lorenz, and Weber determined directly the value of the mercury-unit, and the values assigned to the ohm on their authority are calculated by the ratio just given. Rowland determined the absolute value of copies of the ohm. With regard to the experimental methods by which these numbers were obtained, it may be noted that, with the exception of one out of

three different methods employed by Weber, they all depended in one way or another upon the production of induced currents. The exceptional method depended on the determination of the rate at which heat was evolved in a conductor traversed by a current of known strength, a method that had been employed by v. Quintus-Icilius in 1857, and by Joule in 1867, for the determination of the mechanical equivalent of heat. Lorenz's method calls for a moment's remark on account of its being the only one of the induction methods in which a steady current was employed, and from the great simplicity of the theoretical principle involved. Resistance in absolute electro-magnetic measure is represented, just as velocity is, by the ratio of a length to a time, and in Lorenz's method the only measures which enter into the final result are certain linear dimensions and the time of rotation of a copper disc. It will be seen that his results differ from those of other observers to a relatively large extent, and it is understood that they were given by him rather for the sake of establishing the practicability of his method than as laying claim to a very high degree of accuracy.

But even leaving Lorenz's numbers out of account altogether, a comparison of the remainder shows that the real resistance of our practical standards of resistance is not yet known with all the certainty that is desirable. Happily we may hope before long to have further evidence on the subject, Dr. Schuster having, I understand, been engaged for some time past at the Cavendish Laboratory, Cambridge, in redetermining the value of the ohm with the original instruments of the British Association Committee, under the superintendence of Lord Rayleigh. A revived Electrical Standards Committee was also appointed by the British Association at its late meeting at Swansea, and one of the duties of this Committee will be to make another determination with independent apparatus and by a somewhat different method.

From time to time doubts have been expressed as to what evidence there is that any coil now in existence represents accurately the resistance of the ohm as fixed by the original Committee. As a precaution against the effects of a possible secular change in the specific resistance of wires, the Committee

embodied their results in a number of coils of different materials, it being reasonably assumed that any serious change of the kind mentioned would reveal itself by affecting the different metals unequally. These coils, which were originally deposited in the Kew Observatory, and latterly in the Cavendish Laboratory, were compared together by Mr. Hockin in 1867, with the general result that there was no certain indication of change in any of the wires. In 1876 seven of the same coils were re-examined by Professor Chrystal, whose results generally confirm those of Mr. Hockin, except as to a wire made of platinum-silver alloy, the resistance of which seemed to have slightly gone down. Again, during the last two years, Dr. J. A. Fleming, of St. John's College, Cambridge, has been engaged in a very careful study of the temperature-coefficients of the same seven coils, and he tells me that five of them agree very closely indeed at the temperatures marked by the Committee, while the other two (one of them being the coil which Professor Chrystal had previously found to disagree with the others) differ somewhat from the rest, but that they "all are well within a $\frac{1}{1000}$ of an ohm of each other."

Of scarcely less importance, both scientifically and practically, than accurate standards of measurement, are exact methods of comparison. I do not propose to occupy your time by discussing details of any such methods, but there are two which have proved to be capable of such very various applications, and which illustrate so well the point I have had in view all through my address, namely, the mutually beneficial influence exerted on each other by pure and applied science, that I venture to refer to them. I mean Wheatstone's bridge and Poggendorff's method of comparing electro-motive forces. Without making any attempt to point out nearly all the applications of either of these methods, I may mention some of those which are perhaps of greatest interest to practical electricians. Let us begin with the Wheatstone's bridge. Of the four resistances, replace two that are connected with the same pole of the battery, by two condensers, and we have Mr. De Sauty's arrangement for comparing inductive capacities; replace by condensers two resistances connected with the same galvanometer-terminal, and we have an arrangement for the comparison of

inductive capacities communicated to this Society by Sir William Thomson (*Jour. Telegr. Eng.*, Vol. I., p. 397, 1872); replace all four resistances by condensers, and we have another arrangement of Sir William Thomson's for the same purpose. Another modification gives us a method devised by Mr. Mance for the measurement of battery-resistances. Lastly, if two resistances connected with the same battery-terminal are replaced, one by a telegraph cable, and the other by an equivalent artificial line, we have a practically successful method of duplex-telegraphy.

Poggendorff's method of comparing electro-motive forces has not given rise to so great a variety of applications, but the principle is of constant utility in experimental work. By slight modifications it yields two distinct methods of measuring battery resistances;* and as improved by Mr. Latimer Clark it became the "potentiometer," and, as Mr. Clark has shown, in combination with a standard cell of known electro-motive force, it gives a method of determining the strength of currents in absolute measure which is probably much more accurate than the use of an ordinary tangent-galvanometer.

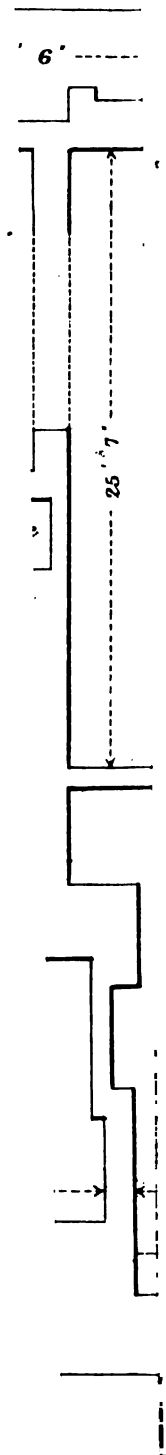
There is a very important service of a different kind from any that I have spoken of as yet, which has been rendered to science by practical electricians. I have dwelt at, I fear, tedious length on the scientific value of an absolute system of measures, and upon the way in which the adoption of such a system in connection with electricity has been hastened by the requirements of practical electricians. I have not, however, mentioned one circumstance that has greatly facilitated the introduction of the system. I mean the selection by Sir Charles Bright and Mr. Clark of decimal multiples of the primary units of such values as are convenient for practical use, and the sending of each of them out into the world with a simple well-chosen name, such as *ohm*, *volt*, or *farad*. It is certain that the practical unit of resistance would have had long to wait for the honourable recognition it now enjoys if it had had no handier name than "ten million metres per second." It is said that

* One of these described by Mr. Mance in 1871 (*Proc. Roy. Soc.*, xix., 252, 1871) had been published four years earlier by M. Raynaud (*Compt. rend.*, lxv., 170).

literary men never invent a genuine new English word—they can only borrow one ready made from Greek or Latin. It is the people who find themselves actually doing and working with new things who are able to invent new English words like *tram* and *shunt*. In science the constant growth of ideas causes a constantly-recurring need for enlarged powers of expression. To supply this need in one direction, Professor Everett introduced, 7 or 8 years ago, those useful words *dyne* and *erg*, which seem now to have taken firm root, but they would hardly have come into existence had it not been for the encouraging example of the *ohm* and *volt*.

Let me end with a practical suggestion, which is perhaps all the more likely to be acceptable because it is not altogether original. From time to time one hears complaints of the want of agreement between the "resistance boxes" issued by different makers, or even that those issued by the same makers are not consistent with each other. Such irregularities, if they exist to any considerable extent, are of course a very serious evil, but at present there is no recognised and accessible authority to appeal to in such a case. It has even been suggested that the uniformity of electrical standards is of sufficient importance to the community for them to be taken cognizance of by the Government Standards Department. But this, however desirable eventually, can hardly be asked for until scientific men are more closely agreed than they are at present as to what the absolute value of the standards ought to be. In the meantime, there is no body in the kingdom which could so suitably take the supervision of this matter as the Society of Telegraph Engineers and of Electricians. Might not an arrangement be made—something on the model of the system followed at the Kew Observatory for the testing of meteorological instruments—whereby any one who wished it might have his resistance-coils compared with some one standard, and might receive a certificate under the authority of this Society, stating, with sufficient accuracy for practical purposes, their errors as referred to this standard? Whether the organisation required for this purpose would be practicable, I cannot say, but it seems to me that the plan is at least worthy of consideration.

Mr. E. GRAVES: I think that the Society has good cause to congratulate itself on the fact that the diversity of Presidents who have occupied our chair have treated us to addresses founded on almost every conceivable branch of knowledge to which their attention could be directed, and that no President has trodden in the steps of a predecessor and reproduced anything bearing on the same ideas. We have listened to many eloquent and interesting addresses, and I am sure you will all agree with me that the worthy President who now occupies the chair for the first time—and who, I am sure, all will heartily welcome among us—has given us an address inferior to none that we have hitherto heard. It is not easy, by simply listening to an address read for the first time, to immediately fit oneself to attempt to characterise it. It strikes me, however, as possessing several features of special importance—as being a *resumé* of those successive scientific discoveries (to use his own words) "of a qualitative" character that have aided in the practical development of applied science, and it has shown us how the labours of the closet and the laboratory have conduced to the commercial success of apparatus of all kinds used at the present day. It has shown us how the practical use of the principles originally discovered in the closet have enabled some conclusions of the discoverers to be corrected, and it has shown us also the great necessity for exact calculation, so that, given a certain electrical arrangement, the result can be predicated as exactly as a naval architect by the use of certain measurements, dimensions, and laws can predicate the stability or speed of a vessel. From the very multifarious nature of the topics dealt with by Professor Foster, it is impossible to form more than a very imperfect idea of the value of the paper we have listened to; but I am perfectly certain that it will be a most important subject for study when we have the opportunity of reading it; and I am sure you will all join with me in the resolution I have to propose, viz., "That the best thanks of the Society are due to the President for the able and lucid address just delivered by him, and that, with his permission, the same be printed and published in the Journal of the Society."



Professor D. E. HUGHES seconded the resolution, and said he was sure that every one present would heartily approve.

The resolution carried unanimously.

Professor FOSTER: I have to thank Mr. Graves and Professor Hughes most cordially for the very kind manner in which they have proposed and seconded the vote of thanks, and the members present for the way in which they have received it, as well as for the indulgence with which they listened to my address.

SOME EXPERIMENTS ON INDUCTION WITH THE TELEPHONE.

By A. W. HEAVISIDE, Member.

During December, 1880, whilst making some experiments on induction, in which the telephone was used as a current detecter, I experienced much trouble at first from foreign induction. The experiments were made in a room where there were many batteries and wires leading to the instrument room, and line wires from the instrument room to the street pipes.

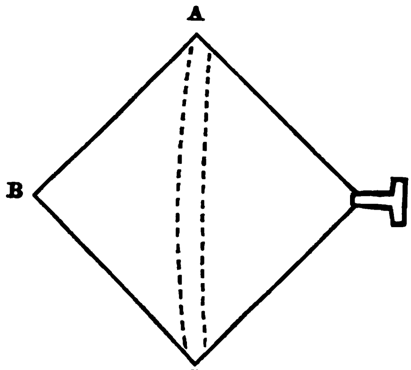
The difficulty that presented itself was how to distinguish what was general from the particular induction being experimented upon, and in endeavouring to separate and find the value of each I was led to make the following experiments:—

The accompanying plan shows the disposition of the basement rooms in which the experiments were conducted.

1. In the position shown in the plan (Fig. 1), it was found that, if about 18 feet of G.P. wire (18 copper-covered to No. 7) was opened out and suspended by means of loops of gutta percha strip at the three points A, B, C, and the extremities connected to the primary of the small induction coil of a Crossley's transmitter, a Bell telephone being in circuit with the secondary coil of the same, induction from the neighbouring batteries and wires was plainly heard, so plainly that Morse signals by hand could be heard sufficiently loud at times to be read, whilst automatic transmitter signalling could also be heard.

Upon closing up the G.P. wire, as shown by the dotted lines the sounds ceased.

FIG. 2.

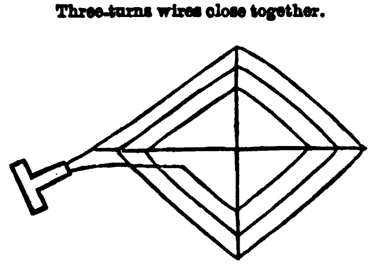


2. The telephone was now joined up minus the induction coil, and the sounds were diminished to about one-third the loudness.

To prove that the sounds were not due to leakage at the points of suspension (for I had previously found surface leakage to produce disturbing effects), an electroscope was put in contact with the G.P. wire and was not discharged.

3. Following the example of Professor Hughes, who has made this field of study particularly his own (March 12, 1879), a light frame for the support of the wire was now made, two pieces of lath tied together in the middle so as to form a cross, each piece 6 feet long, and the G.P. wire was fastened to it as here shown, thus making a very portable coil.

FIG. 3.
Three-turns wires close together.



The induction coil was used as in experiment No. 1. On

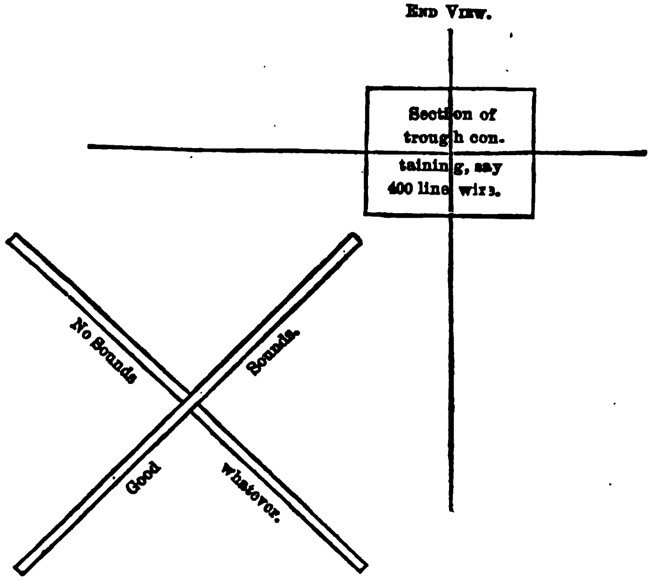
carrying this induction current detecter about the battery room, the echo of what was going on in the instrument room was plainly heard.

The induction coil was now, for convenience, discarded (although this somewhat reduced the sounds), and the telephone was joined direct to the wire on the frame. With this arrangement, on placing one side of the square against any set of batteries, if the batteries were in use two kinds of sounds were distinguishable, one the general induction of the neighbouring wires and batteries, the other the particular induction of the battery parallel with which the frame of wires was held. In the latter case all the messages passing could be distinctly read.

4. Upon taking the frame and placing it parallel with the trough containing the line wires entering the building, the general induction was very distinct, and the theoretical laws of induction were well illustrated, a neutral position always being found when two sides of the wire frame were equidistant from the centre of the

Fig. 4.

End View.



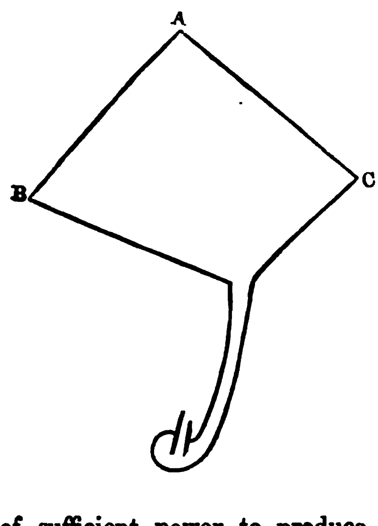
trough; at all intermediate positions the sounds varying with the angle in which the frame was held.

The loudest sounds were heard when the experimental coil was so held that the trough and the coil were in the same plane, and turning the frame round to any other position always caused a diminution in the sounds. The neutral position in which all sounds ceased was when two sides of the frame were equidistant from the trough and parallel to it.

These experiments were varied in many ways, with results all confimatory of those described, and then a new fact presented itself, for I found that the induction produced sounds in the telephone when disconnected.

5. The wire suspended by G.P. loops at A, B, C was connected with a contact breaker giving 100 reversals per second,

FIG. 5.



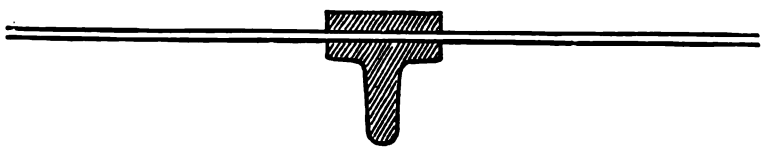
and a battery of sufficient power to produce a current of the strength of 2 Webers. On taking a disconnected telephone anywhere within 15 feet of the wires A, B, C, the reversals could be detected; on approaching the wire they became very loud, and when a turn of wire was taken round the neck of the telephone the sounds could be readily heard without placing the telephone to the ear. Of course, when the telephone was placed anywhere within the space bounded by A, B, and C, the sounds were very loud.

6. This experiment also very beautifully illustrated the laws

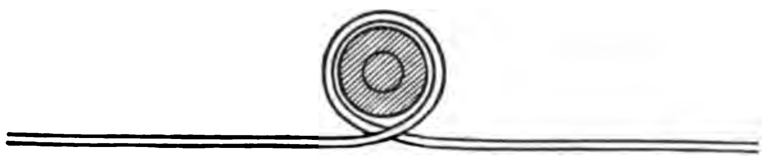
of induction, inasmuch as if the telephone was placed as in position 1, loud sounds were heard;
2, louder sounds;
3, no sounds whatever;
4, ditto.

Fig. 6.

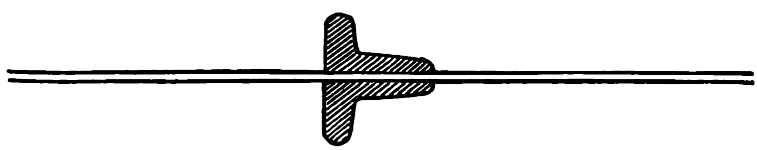
1.



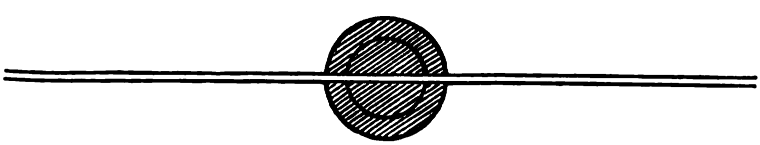
2.



3.



4.



On taking a disconnected telephone and coiling one turn of any battery connection round it, if the battery was in use all that was passing could be heard, and the messages read if sent by hand.

7. I next found, if the terminals of the telephone were connected together, and a conductive closed path were thus offered for the induced currents, that, instead of obtaining *louder* sounds, the sounds were amazingly reduced—smothered, in fact.

This fact enabled me in the experiments I was conducting to distinguish when I heard sounds whether the currents I was endeavouring to detect existed, or whether the sounds arose from

induction from the primary or surrounding batteries; for if there was any uncertainty, by removing one of the connections, the sound either disappeared entirely or increased in loudness.

In the former case the sounds must have arisen from induction acting on the wires in connection with the telephone; in the latter, from direct induction on the telephone itself, proved by the increase in loudness on disconnecting it.

The only case of induction on a disconnected telephone producing sounds with which I became acquainted, through looking up the subject after making these experiments, was the published observations of Mr. Stroh, who found that a powerful magnet moved close to a disconnected telephone caused it to emit faint sounds. Mr. Stroh has, however, since informed me that Professor Hughes has observed that a disconnected telephone is sounded by the making and breaking contact of an induction coil at work in its neighbourhood, and that he (Mr. Stroh) has verified this experiment, and finds the sound audible in an adjoining room.

Why the sounds are so much louder when the telephone is disconnected than when it is short-circuited may perhaps be thus explained. We have a primary wire in which a current is made and interrupted, and a secondary wire (the telephone coil) in which the makes and breaks of the current in the primary induce electro-motive forces. When the secondary is closed, the induced current due to the establishment of the primary current is a continuous wave, rising to a maximum and falling to nothing again. But when the coil is disconnected, a continuous current cannot flow, and the induced current is oscillatory. Its magnitude is, of course, immensely less than in the former case, but its oscillatory character renders it more suitable for producing sounds.

Mr. O. Heaviside informs me that the period of the induced oscillatory current in a secondary wire (where the terminals are connected to a condenser of small capacity) due to the establishment of a current in a primary is represented by the following formula:—

$$2\pi\sqrt{c\left(L_2-\frac{M^2}{L_1}\right)}$$

where c is the capacity of the condenser, L_1 the coefficient of self.

induction of the primary, L_1 that of the secondary, and M their coefficient of mutual induction; the frequency being thus inversely proportional to the square root of the capacity of the condenser. The disconnected telephone may to a first approximation be regarded as connected to a condenser of small capacity.

Addition, January 25th, 1881.

The following experiments have reference principally to the effect produced by connecting condensers to the disconnected telephone.

The sounds in a disconnected telephone produced by making and breaking a primary circuit containing a battery are, as might be expected, considerably increased by, instead of a single turn, making the primary make several turns round the telephone.

Winding four feet of the primary into a flat coil, and laying it flat upon the mouthpiece of the disconnected telephone, gave the same result.

Equally loud sounds were obtained by wrapping four feet of the primary round the neck of the telephone close to the coil, and in all cases the sounds were much dulled by short-circuiting the telephone.

The effect of electrostatic capacity is curious. A single turn of the primary being taken round the neck of the disconnected telephone, and condensers of different capacities being attached thereto, I found in the first place that a capacity of about $\frac{1}{1200}$ microfarad made no perceptible difference, ditto with $\frac{1}{200}$ microfarad.

With a capacity of from $\frac{1}{8}$ to $\frac{1}{4}$ of a microfarad, the sharpness of the sounds was increased, but further addition to the capacity produced diminution, which continued until with 29 to 30 microfarads the sounds were the same as with the telephone short-circuited. On gradually taking out capacity, the sounds increased until the normal sound of the disconnected telephone returned; and I again found that a capacity of from $\frac{1}{8}$ to $\frac{1}{4}$ microfarad increased instead of diminishing the sharpness of the sounds.

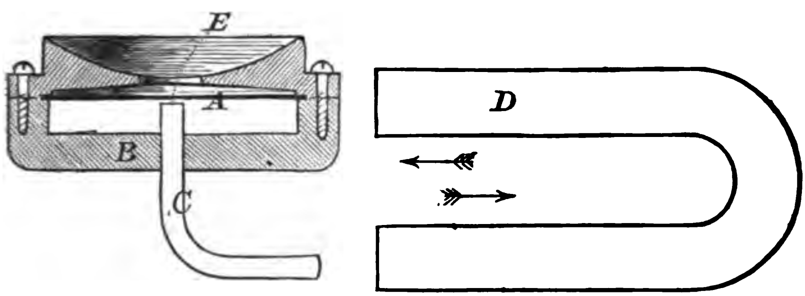
Next, with resistance coils connecting the terminals of the telephone instead of condensers, I found that the insertion of 10,000

ohms gave nearly the same sounds as disconnection, while smaller resistances diminished the sounds, which became gradually smothered as the resistance was reduced. These coils were, of course, double wound. An electro-magnet instead quite altered the character of the sounds by its self-induction, producing, in fact, a similar effect to what is to be observed in the Gower-Bell telephone. In receiving with that instrument, the secondary of an induction coil is in circuit, but there is an immediate improvement both in loudness and distinctness if the secondary coil be short-circuited.

Professor D. E. Hughes: I have listened with a great deal of interest to Mr. Heaviside's paper, and I must say that I have myself observed many of the effects mentioned, and can vouch to their accuracy. The reason why the effect was greater in one experiment than another was because a quantity wire was used. When an intensity wire was substituted for a quantity wire, the induction became stronger. The employment of a square coil allows us to observe the effect upon that portion of the coil nearest to the supposed cause of induction, and the larger the square coil used the less neutralising effects we perceive from the opposing side of coil. The diagrams used by Mr. Heaviside illustrate this. The same effect takes place in the induction balance. If a piece of iron be exactly in the centre, no effect is produced, but the slightest deviation will cause a positive or negative induced current.

The paper deserves great credit and thanks.

Mr. A. Stroh: As Mr. Heaviside mentioned in his paper an experiment made by me some time ago (described in the *Telegraphic Journal*, 15th Jan., 1879), I should like to say a few words respecting it. It is a fact that, if a powerful magnet is made to approach a telephone while the latter is applied to the ear, sounds are heard which resemble the breathing of a person or child. That is entirely a magnetic effect, which I have endeavoured to investigate further. The following diagram illustrates the experiment:—



It practically consists of a thin iron diaphragm, A, and a wooden crossbar, B, supporting a bent rod of soft iron, C. When a strong horseshoe magnet, D, is brought into the position shown, the diaphragm, A, and the rod, C, become magnetised, but with opposite polarity, and attraction takes place at E. If now the magnet, D, is caused to move gently to and fro in the direction of the arrows, the diaphragm, A, will make corresponding movements, approaching and receding from the iron rod, C. During each of these movements the sounds I have mentioned can be heard. It further appears that the quality of the sound during the approach of the magnet, D, is slightly different to that produced by a movement in the opposite direction.

Mr. Alex. J. S. Adams: Whilst discussing the subject of induction in relation to the telephone, permit me to mention the result of some experiments made upon the effect of circulating currents upon iron. It is well known that if a piece of iron be presented to the end of a hollow coil, the passage of a current through the coil tends to draw the iron into it, and to hold the iron within the coil until the current ceases. This effect is apparently one of magnetic attraction. If, however, a straight piece of iron wire be balanced along the centre of a hollow helix, and is free to move, *at the moment* of completing or of breaking the circuit the iron wire is drawn from its central position towards the inner wall of the coil—in fact, to that part of the wall to which it may be nearest. This effect is but *momentary*, for the wire immediately returns to its original position. This *lateral* attraction on the part of the coil is beautifully shown if fine iron filings be caused to occupy a central position along the inside of the coil,

for the filings will at the moment of completing or breaking the circuit jump and start towards the inner wall of the coil, although owing apparently to want of weight, and to the shortness of the interval during which the effect takes place, they do not materially change their original position. It would seem that this lateral effect is purely one of induction, and the result of the experiment may interest the author of the paper just read.

Professor HUGHES: I will just say with regard to Mr. Stroh's experiment that I have repeated it myself, and would recommend every one possessing a telephone to repeat it also. I have tried to investigate the cause of the sound produced, but my place is too noisy to admit of sufficient stillness to trace it, though it is evidently due to the magnetisation in the telephone not being continuous. If it were we should not have "breathing." The experiment is a most beautiful one, and will in my opinion lead to something greater if properly investigated.

Professor AYRTON: I am not quite clear whether Mr. Heaviside in his paper gave us one reason why he heard better when the telephone circuit was open than when it was closed. Does not the primary battery current act directly on the magnet of the telephone and not on the coil? Of course, we hear the telephone simply because there is a change in the magnetism of the bar magnet, which causes motion of the diaphragm. We know that in an ordinary Rhumkoff coil the main object of the primary current is not to act directly on the secondary coil, but by its intermittence to produce changes in the magnetism of the bundle of iron wires which produce the currents in the secondary coil. If we consider that we have a primary wire through which there is an intermittent current produced by a battery flowing, we should have changes produced in the magnetism of the bar, which would produce corresponding actions of the telephone, and so sound would result. The same, to a certain extent, would happen if the circuit were open. But if a closed circuit were used, two inductions would have to be considered, viz., the direct induction of the primary current (when it is altered in intensity) on the magnet, and the induction of the primary current on the closed secondary current, which itself again acts on the magnet. When

a current flows along the primary coil it tends, say, to increase the magnetism in one end of a magnet, but that current would induce, in the opposite direction, a current in the secondary coil, and that current would tend to diminish the magnetism of the magnet: that is, the two effects would tend to neutralise one another, and consequently there would be less change in the magnetism of the bar, and therefore less motion of the diaphragm than there would be if the coil of the telephone were open or removed altogether. If this be correct, then the best effects ought to be obtained if there were no coil in the telephone at all; not so good if an open coil be used; and, lastly, the worst effects with a closed coil.

I mention this, as it does not appear to me that Mr. Heaviside has pointed it out in his paper.

Professor HUGHES: Excuse my rising again, but I wish to say that I can corroborate the remarks Professor Ayrton has just made. I have tried the experiments, and he is quite right in stating that the effects are far greater without the coil. I use, in fact, myself a telephone without a coil to test my batteries; and it is a fact (which I published in the *Comptes Rendus* in Paris last year) that with a diaphragm and magnet placed at one ear and a coil at the other ear, all that is passing in the coil will be heard from the diaphragm; the head, as it were, acting as a transmitter.

The PRESIDENT (Professor FOSTER): I think there can be no doubt that Professor Ayrton has given us the correct explanation of the effect of closing the circuit. The direct action of the primary current upon the coil of the telephone must be extremely small; it can only be due to the difference of distance between the two edges of the coil, one being nearer to the primary than the other. The effects of the primary circuit on the iron of the magnet must evidently be much greater than its direct effect upon the coil. I should like to ask Mr. Stroh a question about the experiment he has mentioned to us. I did not at first sight see why the bit of soft iron wire should be bent on one side, but I presume that that is in order that the force of the magnet should not act directly upon the diaphragm? Also as to the precise way in which the experiment is made, whether the magnet is held in

the hand when it is brought near, because this "breathing sound" means that the disc must be making minute vibrations during all the time the sound lasts. I should think it is almost impossible (even though we think we are holding it steady) to prevent small movements in the hand which would cause one pole to be continually approaching or receding. Holding the magnet in the hand must cause a slight trembling, but that explanation would go for nothing if, when the magnet is held artificially steady, the sounds are still perceived equally well. Let me ask how far anything of this kind is possible towards explanation?

Mr. STROH: I think I can satisfactorily answer Professor Foster's questions if I may refer once more to the diagram. The first time I tried the experiment I used an ordinary Bell telephone with a straight bar, and found that, on coming near a strong magnet, almost any movement produced the breathing sound; but the arrangement I have described to-night with the bent iron bar or rod gives the best effect. By this arrangement the magnet, D, can be brought easily into such a position as to induce magnetism in the diaphragm and the iron rod at the same time. The experiment is performed by simply moving the magnet to and fro with one hand near the instrument, which is held to the ear by the other hand. Respecting the suggestion that the sounds may be caused by an unsteady hand in moving the magnet, I will say that I think it quite impossible, with a horseshoe magnet weighing about two pounds, to produce movements quick enough so as to cause sound. Any one who may repeat the experiment will at once perceive that the effect is not due to unsteady movements of the magnet.

As to the origin of these sounds, it seems to me that the diaphragm, when drawn towards or receding from the iron rod, does not perform these movements with smoothness, but by numerous minute and irregular jerks. But whether the gradual increase and decrease of magnetism in the iron rod and diaphragm takes place in jerks or step by step, or whether it be in the nature of an iron diaphragm to yield to a gradually changing force by minute jerks, I have not been able to ascertain. Certain it seems that, if the diaphragm moved smoothly and gradually, no sounds would be the result.

A vote of thanks was then accorded to Mr. Heaviside, and the President announced that the ballot for new Members and Associates was opened.

The following gentlemen were elected

As Foreign Members:

Paul Bayol.
Dr. Mathues Nogueira Brandão.
Gavino R. Cueli.
F. I. C. M. Holst.
N. Ispolatoff.
F. Kolvig.
S. Lauritzen.
C. A. Möller.
E. B. Petersen.
N. Pissarewsky.
Olegario V. Ugarte.

As Members:

Ernest Buller.
A. Ainslie Common.

As Associates:

William Belville.
George Menzies Clements.
William D. Gooch.
Frederick Edward Hesse.
James Huggins.
Alfred Richard Sennett.
Henry Taylor.

The Ninety-fifth Ordinary General Meeting of the Society was held on Thursday evening, February 10th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. FOSTER, F.R.S., President, in the Chair.

The SECRETARY read the Minutes of the last Meeting, which were confirmed, and the names of new candidates were announced, after which the following paper was read:—

EARTH CURRENTS.

(*Second Paper.*)

By ALEXANDER J. S. ADAMS, Associate.

In bringing the subject of earth currents under your notice this evening, I would, in the first place, incidentally refer to my previous paper upon the same subject; and, in the second place, lay before you some important data with reference to the so-called "diurnal" variation, submitting also an analysis of the results of my own investigations regarding that particular phase, together with deductions and inferences that, to my mind, appear to be the logical outcome of the analysis.

In my previous paper it was asserted that the *normal* earth current was clearly distinguishable from the temporary, stronger, and more variable currents that break in upon it, and which I have called "Electric Storms," and that whilst the former appeared to be obedient to some general law, the latter was evanescent and exceptional. In the light of more recent experiences, and because of the extreme complexity of the subject, I would again ask your recognition of that distinction.

Moreover, in that paper the absolute value of the electro-motive force of the normal earth current passing over a wire running parallel to the line of greatest force was stated rarely to exceed a strength equal to five Daniell cells per 150 miles; but, from a careful daily record extending over the past two years, it appears that the usual strength of the earth current at the maximum of its ordinary variation rarely exceeds the one-tenth of a milliweber.

In the present paper I propose to confine myself to the normal currents only; and before proceeding to examine the actual results of my investigations, I would ask your permission to state some of the reasonings, based upon the recorded impressions and results of fifteen years' labour, which have led up to, and guided me in my conclusions.

I regard the globe we inhabit as essentially an electrified sphere, its electricity being liable to disturbance both from within and from without; whilst the known fact that the electrical disturbance of a sphere is calculated to increase, as it were, its electricity upon the sides *perpendicular* to the disturbing force—as, in fact, the tides act in reference to the moon—is equally referable to disturbances of the normal electrical condition of our globe.

At first sight, following up this idea, it appeared that the probable cause of electrical disturbance as regards our globe was the sun, but investigation showed that the mean direction of the normal earth current, as given upon the chart furnished with my last paper, in no way coincided with the lines of solar influence, whilst the daily maxima and minima of current were indefinite, the variations of one day rarely agreeing with those of another, and the mean of one series of days often disagreeing with the means of other series: indeed the evidence was altogether against a theory of *solar* causation for these normal earth currents.

To elucidate the question more thoroughly, I commenced in 1878 an analysis of the observation figures furnished by Mr. James Graves (member of the Soc. Tel. Engineers) in 1873, with a result, so far as the analysis was carried, that the cause was evidenced as being distinctly *lunar*, and the variations *lunar*-diurnal. The curves of these figures, however, appeared to be much interfered with by cable fault-currents of polarisation, and other local currents, so that I availed myself of an opportunity which offered in March and April, 1879, for prosecuting a further systematic course of observation, and those observations were, by means of a wire, the ends of which were connected to the earth, one at Cardiff and the other through a sensitive astatic galvanometer at London, and with the kind assistance of confrères and telegraphists* obtained

* Messrs. W. Morrison, R. Lewis, C. Hibberd, and W. Goulding.

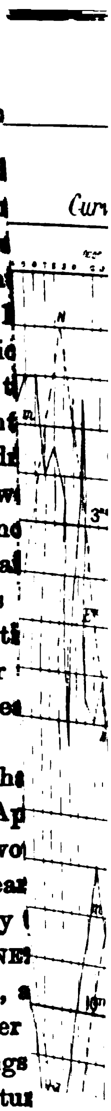
every quarter of an hour from March 28th to April 26th, wit exceptions, and the curves of the figures so obtained a remarkable for their regularity as for the clearness with they indicate the *lunar* origin of the variations they represen

Plate 1 represents consecutive days from the 1st to the 21 April, inclusive, each day being divided by thin perpendic lines into hours coinciding with Greenwich mean time. The horizontal lines marked —●— represent lines of no curren distances above and below which, thin horizontal lines are d representing degrees of current in milliwebers; for the milliw value of each degree of galvanometer scale being previously kn the value of any earth-current deflection was at once obta without further calculation. The dotted curve represents moon's phases for the dates given upon the diagram, *i.e.*, her ti of southing for London, and of London's arrival at the nadir nodes of her orbit; whilst the thick curve represents the e current variations observed during the same interval.

Plate 1, therefore, is a comparison between the moon's ph and the earth-current variations from the 1st to the 21st Ap 1879, and it will be seen how very nearly they coincide. I wo here suggest that the importance of the fact that the normal ea current variations of our globe are almost wholly controlled by moon, can hardly be over-estimated; because, whilst the MAGNE variations at different localities are more or less complicated, a are influenced apparently by more than one disturbing power shall later on endeavour to show why from the nature of things should be so), the consideration of the earth's ELECTRICAL distu ance is lifted from mere particulars or accidents to more gene principles. A due apprehension of this assumption: that t *moon's influence is, in proportion, felt by the earth's electricity every part of the earth's surface,* is necessary for the proper appr ciation of the deductions which will hereafter be submitted.

There is, however, a deeper meaning in the lunar-diurnal curv than would at first sight appear, and we will analyse that of on *lunar* day by the aid of Plate 2.

Of course our lunar day is the mean result of the motions c the earth and moon, although from the surface of the former th



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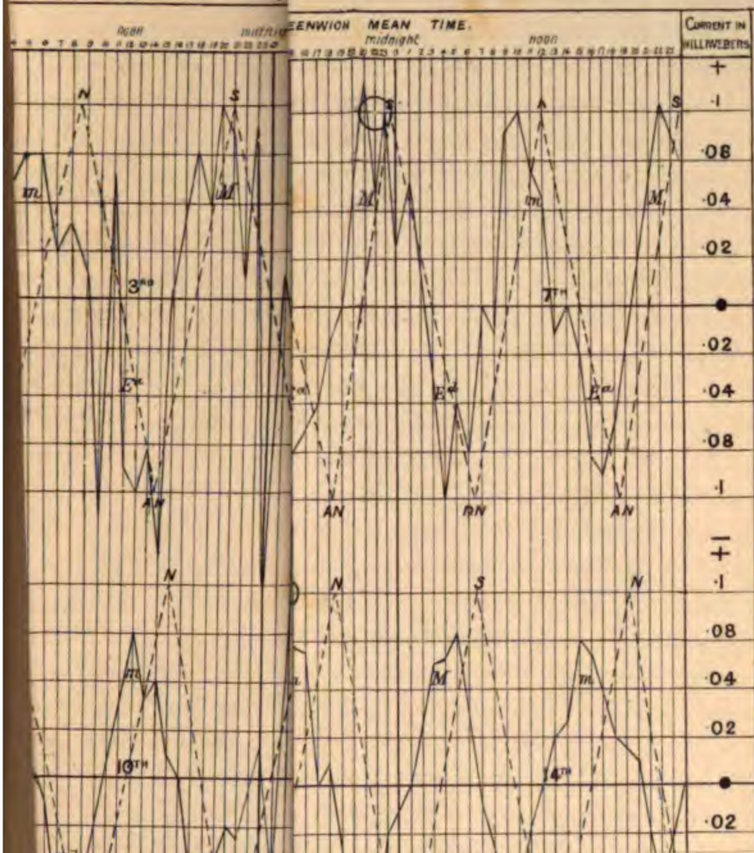

Plate 2.

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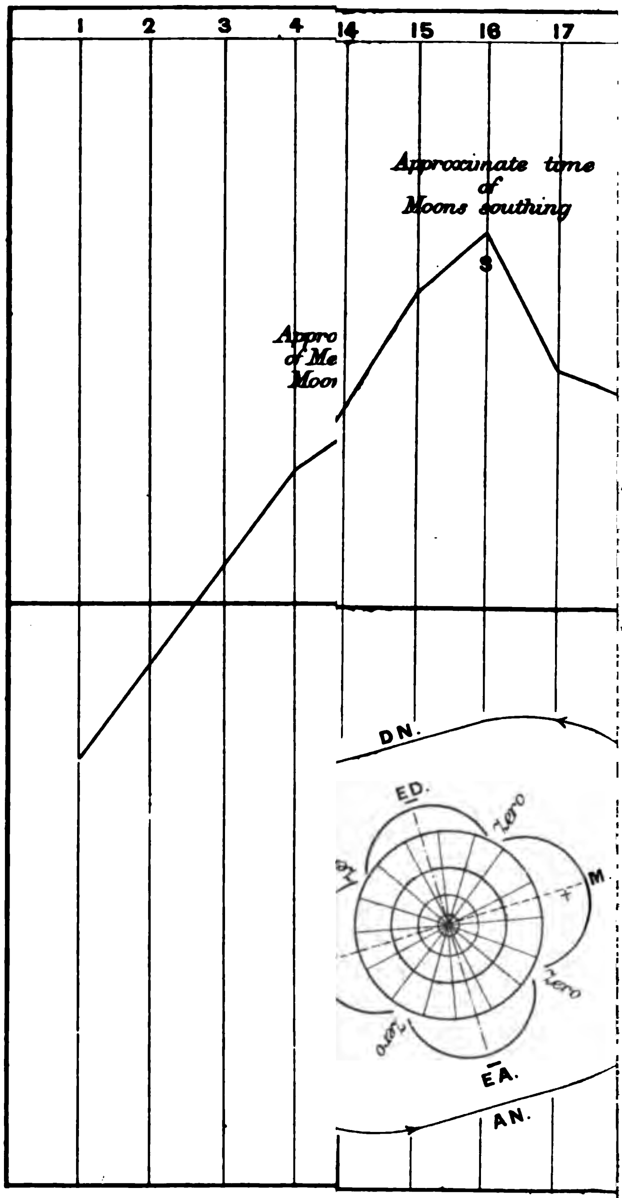

V A n appears to travel round the earth once in about 24 hours; to enable us to comprehend more easily the signification of the ar-diurnal curve, let us take it for granted that, whilst the moon the time being is stationary, the earth, by her rotation, presents 17 eenwich during the lunar day successively to all parts of the ar orbit. Following Greenwich in her day's journey, we find selves at starting directly opposite the moon, and, at the same time ne, in a position of maximum inductive influence or point of ongest current, as at + M (Fig. 1, Plate 2).

up Circling onwards, Greenwich, in about three hours, reaches an ectrical zero or point of least current; and three hours later still, hen opposite the descending node, DN (Fig. 1, Plate 2), *a second maximum*, —ED, *but of different kind*, is experienced. Another hree hours, and a second zero is passed, and again three hours brings Greenwich opposite the moon's nadir, and also to a maximum *similar in sign to the first*, + *m* (Fig. 1, Plate 2).

Precisely similar features obtain for the other half of the lunar day, and the result of this analysis as regards the current phases of one lunar day is equally true of any other lunar day in the year.

If, then, these earth-current phases obtain for Greenwich, why not for all other points upon the same meridian? I take it as altogether illogical to suppose that such current phases obtain ONLY upon that portion of the meridian at which Greenwich happens to be situated. May we not rather conclude that for a given time, a similar development of the earth-current obtains throughout? And if this be so, and I take it to be, it simply means that in passing the *lateral* maxima, —ED, —EA (Fig 1, Plate 2), Greenwich cuts through a *ring* or *belt* of maximum electrical effect that encircles the earth mid-way between those points of the globe which are respectively opposite the moon and the moon's nadir. This circle of maximum electric effect is illustrated at Plate 3.

But there are the four zeros for consideration, because each of the four maxima of a lunar day is divided from the others by them, as shown upon Plates 1, 2, and 3. A careful examination of the facts will hardly fail to convince us that here also we have *zones* or *belts* of zero extending round the globe, one upon either side of the *electric-circle*, and dividing it from the electric maxima of the opposite kind, as at ZD, ZA, and *zd*, *za* (Plate 3).

This curious development of the earth's electricity into poles + M + *m* and circle ED, EA (Plate 3), by the moon's influence, is most interesting, and a knowledge of the fact can hardly fail to be of importance.

Before proceeding to discuss the apparent motions of the earth's electricity, it will be convenient to state the designations by which, in my investigations, I distinguish the different phases of the earth current, and by which they will hereafter be recognised in my communications.

First. An imaginary line drawn between the two maxima that always obtain upon the sides of the earth that are perpendicular to the moon, M *m* (Plates 2 and 3), I have termed the *electric perpendicular*, of which the maximum *nearest* the moon is the *major*, and that *farthest* from the moon is the *minor* pole.

Second. In like manner, the circle of maximum already alluded to is the *electric-circle*, of which the right half, looking towards the moon, is the *ascendant*, and left half is the *descendant semicircle.*

Third. The points north and south at which these semicircles meet constitute *the poles of the lunar-diurnal electric axis.*

Fourth. The zones of no current that divide the electric-circle from the electric-poles are respectively the *major zero-circle* nearest the moon, ZA, ZD, and the *minor zero-circle* farthest from the moon, *za*, *zd* (Plate 3).

Fifth. The whole of the electric distribution thus arranged by the moon follows that orb in her course, and revolves about the lunar-diurnal axis: this rotation of the system I have termed *lunar-diurnal circulation.*

Sixth. An imaginary line drawn round the earth at right angles to the plane of the moon's orbit, and cutting the poles of the lunar-diurnal axis, and the major and minor poles of the electric perpendicular, is the *electric meridian,* constituting a convenient basis for earth-current phase calculation.

According to the hypothetical arrangement of the earth's electricity already cited, if the moon's age be given with an allowance for retardation, to which I shall presently refer, the electric curve which should be forthcoming at a given time for

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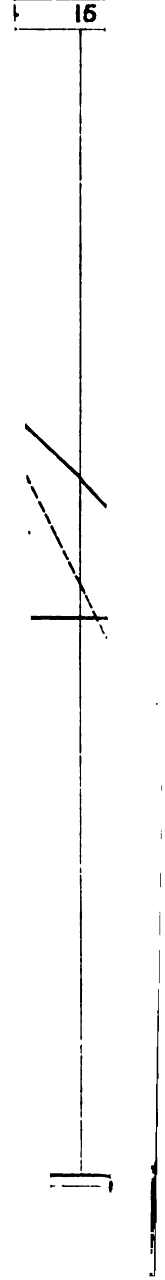

any given spot upon the earth's surface should admit of accurate prediction—a conclusion warranted by the results of one or two experimental trials that have been made.

The *retardation* to which I have alluded is a peculiarity clearly indicated by the variation curves, *as compared with lunar time* (as at Plate 1), and is a lagging of the electric curve some hours in rear of the moon: thus, Greenwich for instance, appears to strike the major pole three or four hours after the moon's precise time of southing for that meridian, and so also as regards all the other electric phases. At first it would seem that this retardation was due in some way to the sun, but upon consideration it will become evident that, if that were the case, the retarding effect would itself be subjected to a regular variation in sympathy with the relative positions of the sun and moon, whereby, when the solar and lunar lines of force were parallel, as at the times of full and new moon, the retarding influence would be minimised, if not quite annulled, whilst when those lines of force were transverse, as at the times of first and last quarters of the moon, the retardation should be maximised; but the result of a series of observations made especially to settle this question conclusively shows that the phenomenon of earth-current retardation is not directly due to solar influence. The indications to this effect are sufficiently clear upon Plate 1.

Another feature noticeable about the lunar-diurnal variation is an apparent enlargement and contraction of the *major pole curve* in sympathy with the height of the moon above the horizon. This peculiarity is ill defined however, and not easily traced; but advantage was taken of that most beautiful of our moons, the *harvest moon*, in September last, to investigate the matter, the result of which is given at Plate 5, where are represented the earth-current curves for the lunar day during full moon in March, when the moon's altitude was not great, and those for the full moon in September, when that orb reached her greatest altitude; and it will be seen that, whilst the March pole and circle curves are nearly equal, there is an extraordinary enlargement of the *major-pole curve*, at the expense of the others, in September.

There is also an indication that a similar expansion arises in

connection with the age of the moon generally; but this feature, together with some others, is still under investigation.

We now come to the consideration of the *apparent* direction of the normal earth-current flow, as obtained by observation. The *direction*, and flow of the earth current have not, so far as I am aware, been yet satisfactorily accounted for. If the mean direction of the earth current, namely, a line running nearly N.E. and S.W., be compared with the plane of the earth's rotation, or with the solar or lunar lines of force, the greatest variance is discernible, (see Plate 4). It is, however, not a little remarkable that if we conceive the earth-current electro-motive forces to revolve about the *electric-perpendicular*, as an axis, in a *contrary* direction to the earth's rotation, and take the mean of the two (*kk*, Plate 4) we obtain a line of directive influence for the earth current, agreeing with that derived by observation. Indeed, as we shall presently see, the evidences are strongly in favour of this circulation of the electric distribution about the perpendicular, and I have termed this presumed motion *terrestrial circulation*, the electric-perpendicular constituting the *terrestrial axis* (Plate 4).

One of the most important results of these investigations is the agreement apparent between the earth-current and the *magnetic* lunar-diurnal variations. Walker,* in his essay, says: "The moon produces a *small*† though sensible variation in each of the three magnetic elements—declination, inclination, and intensity.

"In the case of each of the elements we find a double progression in each lunar day.

"The declination has two easterly and two westerly maxima, and the inclination and total force have likewise two maxima and two minima in the same interval, the variation in each case passing zero four times during the lunar day."

Here we have an interesting and important fact taken in connection with the study of earth currents, the *magnetic* lunar-diurnal curves forming an exact counterpart to our lunar-diurnal current variation (Plate 6).

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* "Terrestrial and Cosmical Magnetism," by Edward Walker. Deighton, Bell, & Cay, Cambridge.

† The underlining is my own.

From a careful examination of the two phenomena in all their bearings, giving due prominence to the fact that, whereas the distribution of electricity on the earth's surface is almost wholly controlled by the moon, the variations of which are strongly developed, whilst the magnetic lunar variations are slight compared with other variations of the magnetic needle, the question suggests itself, may not the *magnetic* lunar-diurnal variation be immediately consequent upon a variation of the distribution of the earth's electricity, in that the earth current acts the part of medium whereby the moon's power is transmitted to the needle? and, moreover, does a similar retardation obtain in the magnetic, as in the current lunar variations? Unfortunately I have no data regarding the latter question, but the *fact* of lunar-diurnal magnetic variations may assist us in our further consideration of the earth current.

It will be obvious that if earth currents take the form illustrated at Plate 3, namely, of poles and circles, the electric-circle and the zero-circles would cross the earth's lines of longitude at an angle of about 18 degrees, so that the most southerly point of any line of longitude will first come under the influence of any given current phase; the more northerly points afterwards coming successively under it at a rate of progression approximately 2·4 minutes for each degree northwards. From this ratio the time at which a given lunar-current phase would reach any point west of, say, Greenwich, after leaving that place, should be deducible, and observations taken at different points upon the same line of longitude would doubtless show this to be the case; indeed, the data extant as regards the *magnetic* lunar-diurnal progression would appear to be conclusive evidence upon this point.

Upon Plate 6 are given curves, reduced from a table of magnetic mean lunar-diurnal variations in declination, for six stations, as computed by General Sabine,* and from these curves we find that the deflections of the needle are precisely those that would presumably occur under the electrical conditions already advanced. Exactly similar magnetic lunar-diurnal variations

* "St. Helena Observations," Vol. II.

obtain in either hemisphere, and in all latitudes, although th appears to be a decided though regular variation in regard to ti as compared with position upon the globe's surface. For examp upon comparing the Toronto and Pekin magnetic curves (Plate we find that the same variation or phase appears to reach Toron from Pekin in about eleven and a half hours, and that when o westerly deflection of the needle (answering to one pole of t electric perpendicular) occurs at Toronto, another westerly defle tion (answering to the other electric pole) obtains at Pekin, an this we should expect to be the case, seeing that within a fe degrees the one place is the antipode of the other.

Similarly we find from these magnetic curves a lapse of abo eight hours between the Cape and Kew, as regards the time which a given deflection obtains, and which, when it is remember that this comparison is between a calculation, upon the one hand and the mean of a number of observations extending over a peri of two years, upon the other, is sufficiently near a calculated lap of five hours for our purpose.

From the evidence I have here sought to lay before you, it wi doubtless appear to be established that the motions of the tru earth current constitute a series of tidal ebb and flow very simila to that of the waters of our globe; the chief difference being that whereas in the case of the ocean there occur *two* flows and ebbs i each lunar day, with the earth's electricity there are *four*— dissimilarity, however, that may yet prove to be more apparen than real.

But a comprehensive consideration of earth-current phenomen opens out a much wider sphere of investigation than that simpl embracing variations of strength: it has to recognise *directiv influence*, which, applied to electricity, means the production o magnetism.

Permit me, then, briefly to point out the electric circulating systems that appear to obtain by reason of these three motions, the *earth's diurnal rotation*, the *lunar current circulation*, and the *terrestrial current circulation*—causes which result in the apparently disconnected variations observable in the movements of the magnetic needle.

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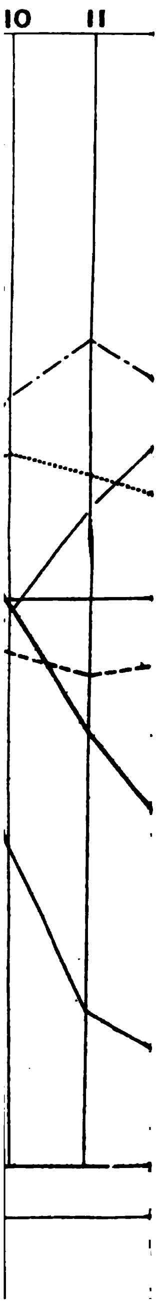
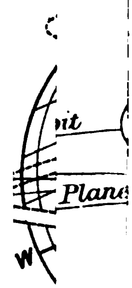




nd, first, we have the earth revolving about her axis, and ing with her her latent, *i.e.*, undisturbed electricity, at a of some 1,400 miles per hour. Now, it is reasonable to con- that such motion develops a strong magnetic effect at the 's axial poles, the precise polarity of which is easily deducible the known law relating to electric spirals. This develop- of *axial* magnetism is illustrated at Fig. 1, Plate 7.

n the second place, we have the earth's disturbed electricity ming round the lunar-diurnal electric axis with the moon, and pleting a revolution once in each lunar month. Here, again, it bvious that further magnetic effects are produced at the poles of lunar-diurnal current axis, the polarity of which will be con- ry to that of the axial. This *lunar-diurnal magnetism* is presented at Fig. 2, Plate 7.

Thirdly, we have the apparent circulation of the earth's ectricity about the electric perpendicular, and a consequent evelopment of magnetism at the electric poles, the polarity of hich is similar to the axial as regards position. This *terrestrial agnetism* is illustrated at Fig. 3, Plate 7, and, curiously enough, n its lunar rotation along the plane of the moon's orbit, is recisely what is required to account for the *magnetic* lunar- diurnal variations.

Fourthly, it will be observed from Figures 1 and 3, Plate 7, that the respective planes of the earth's motion, and of the terrestrial current circulation differ to the extent of about 70 degrees, and hence there would be a magnetic development at right angles to a plane which would be the mean of the two, as illustrated at Fig. 4, Plate 7.

Thus it appears that there are at least four pairs of magnetic poles at the surface of our globe, resulting from earth-current circulation, each and all in degree exerting an influence upon the magnetic needle.

The PRESIDENT: I need scarcely say anything by way of calling attention to the many points of importance and interest in Mr. Adams' paper. No doubt it will give rise to a considerable and interesting discussion. There are many gentlemen present

who have paid great attention to the question of earth currents, and magnetic variation, which is so intimately connected with it, so that we have a good prospect of our knowledge on the subject being considerably increased at this meeting.

Professor W. GRYLLS ADAMS, F.R.S.: I have read with great pleasure the author's first paper on "Earth Currents," which was communicated to the Society some three years ago, and he has followed it up by a paper which we have just heard, and which raises points of very great interest and importance. Mr. Adams has said that our knowledge of the subject is very meagre, but he seems to have thoroughly discussed such evidence as we possess, and therefore may presumably be assumed to be correct in his conclusions as to the main facts, which seem to show that there are regular diurnal earth currents to and fro over its surface, and that the general direction of these regular currents in England is from about S.W. to N.E., and that they are dependent upon something which is directly governed by the motion of the moon. These earth currents are those regular daily currents of small amount which ebb and flow like tides, and the author attributes them to lunar action through the agency of positive and negative electric charges on the earth, the distribution of those charges following the moon in her relative motion with regard to places on the earth's surface. I should have been glad to have seen in this paper more of the evidences on which the author's conclusions are based; but assuming the relations between these earth currents and the diurnal motions of the moon, I think we must hesitate to accept the theory of electric charges and electric distribution, for this theory will hardly account for the results.

Professor Rowland has shown that a statical charge of electricity, when it is carried from one place to another, will produce magnetic effects just in the same way as a current of electricity would do, and therefore the motion of a static charge will produce a current of electricity in its neighbourhood; but Professor Rowland has also clearly shown that the theory which would account for the earth's magnetism and for the changes in the earth's magnetic force, by supposing the earth to have a charge of electricity upon it, is utterly insufficient to produce the observed effects. From my own

study of magnetic disturbances and their intimate connection with auroras and earth currents, I should be rather inclined to look to the regular diurnal changes in the earth's magnetism as the cause of these regular diurnal earth currents. When we get magnetic disturbances in England, we find that they are felt at the same instant of time at Lisbon, at Vienna, and at St. Petersburg, and even at Melbourne, and that accompanying them are auroras and earth currents which occur simultaneously in England and as far away as Siberia. I hold in my hand a letter just received from Dr. Wild of the Observatory of St. Petersburg, in which he writes that on the night of January 31st to February 1st, the very brilliant aurora so well seen in many parts of England was also seen at St. Petersburg and in Siberia. Both at Kew and at St. Petersburg it was accompanied by very violent magnetic disturbances, and we hope to hear from Mr. Preece in this discussion something about the earth currents which accompanied that storm.

This intimate relation between magnetic changes, auroras, and earth current disturbances, rather lead me to consider the regular diurnal earth current of which the author speaks as brought about by more gradual magnetic changes in the earth.

Professor Maxwell has shown us that, with the earth revolving on its axis from W. to E., if an insulated wire were fixed in space outside the earth in such a way as to make contact with the earth at the pole and at the equator, then the revolution of the earth on its axis would cause a current in the fixed wire from the pole to the equator.

If the insulated wire moved over the surface of the earth from W. to E., and the earth did not revolve, or revolved at a slower rate, then the current in the wire would be from the equator to the pole.

The current depends upon the relative motion of the earth and wire. Now the attraction of the moon for the earth not only causes tides in the ocean, but must also cause tides in the earth's solid crust, which is made up of materials which have a considerable degree of elasticity, and must therefore yield and alter their form to a considerable amount under the action of the moon.

We thus have a lagging behind of magnetic matter, or an

alteration of form giving a relative motion of magnetic matter with regard to the wire, whilst the insulated wires on the earth's surface are carried on from W. to E., and hence we may expect in those wires a current of electricity whose general direction would be from the equator to the pole.

The position of England with regard to the magnetic pole would modify the direction of these earth currents; and it is quite conceivable that these tides on the earth, which are directly due to the moon's attraction, may be the cause of the regular diurnal earth currents, and may account for their general direction being from S.W. to N.E. The lagging of these earth currents behind the position of the moon would also be accounted for by the lagging of the tides. Whether this theory will account for the actual observed earth currents is a matter for future consideration.

Mr. H. C. SAUNDERS: I have listened with very great interest to Mr. Adams' paper, and also to Professor Adams' remarks. The subject of earth currents has been one that has occupied my attention off and on ever since 1858, and, from my connection with the Eastern Telegraph Company, I have had opportunities of accumulating a mass of data on cables much greater than any one else can possess. I have handed to the President some diagrams which follow out Mr. Adams' curves most remarkably. In a diagram [held by the President] of observations taken on the Eastern Telegraph Company's cable between Suez and Aden, every 15 minutes for the month of January, 1871, there is a remarkable coincidence in the general contour of curves between noon and midnight, and midnight and noon. The earth current, as a rule, is at zero at midnight, at 6.0 a.m., noon, and 6.0 p.m., and that is followed out very much by the idea expressed by the curves in Mr. Adams' diagram.

In 1858, acting under instructions from Mr. Whitehouse, who was then the Electrician of the Atlantic Company, I went to Valentia, and observed earth currents on 300 miles of cable lost in the previous year for 6½ days (day and night). I only came across the papers a few weeks ago, having forgotten that I had copies of them; but I then found the curves, and I look upon

them as the first earth current ever curved from observations on a cable certainly of the same length. Another remarkable thing is my having plotted the tidal curves. I seem to have had an idea that the maximum and minimum of the earth currents agreed in some measure with the high and low water mark. The high and low water were taken very roughly by observation. I did not satisfy myself as to the age of the moon—I merely put in pegs by which I could know when the water was high and low, and I plotted the red mark shown on the diagram for 36 hours to indicate high and low water mark of the tide. However, upon plotting the tidal curve for the rest of the time, the coincidence of the curves did not appear to me to be so near as I expected they would from the mere inspection of the columns of figures. But having since joined the separate sheets in one continuous length, there is a very marked coincidence of the two curves, more particularly considering the rough way in which the tide was observed: so that there may be something in them to carry out Mr. Adams' idea. But in regard to observations taken at Suez, where the staff had at that time very little else to do, as some of the cables were interrupted, I got observations from the middle of December, 1870, to the middle of March, 1871. Those observations are plotted and drawn in diagrams; and if the Society would like to accept them, I should be very happy to present them to it. It may be said, why have I not done something all this time? I have for a long time been waiting for some theory to explain the matter, but have never myself been able to find one. However, I now place all the information I can in the hands of the Society, as it may be of use for consultation, and something may come of it. I have not myself been able to do anything except to notice the remarkable coincidence of the curves day after day, and no doubt, if undisturbed by what Mr. Adams calls storms, they would form a smooth and even normal line.

At the soirée in Willis's Rooms in December, 1876, I exhibited diagrams of observations for six consecutive days between Suez and Aden, when the cable was broken about 106 miles from Aden, the cable being about 1,460 miles long. They were taken very carefully on an astatic galvanometer, and a very similar contour

came out, but the remarkable thing was that, after reducing the observations of Suez and Aden to the same time, the coincidence of variations were absolutely correct. As Suez became the maximum, so Aden became the minimum. This followed all through the observations. I have copies of the observations, so that they can be handed with the others to the Society. The curious thing was that the cable was absolutely separated at 106 miles, and yet the same coincidence followed as if the cable had been perfect. I have other observations when the cable was perfect, showing exactly the same results. In calculating the strength of the earth currents, I have always assumed that the current, whether positive or negative, emanated from the distant end of the cable, and in all my calculations have included the copper resistance of the cable in the circuit, and so have given the force in Daniell's cells at the observing end through resistance equal to that of the cable. Perhaps it would have been more correct to have given the potential at the end of the cable, but for purposes of comparison this does not matter. It is sometimes remarked that there is a very high potential at one end of the line and very little at the other. If Mr. Latimer Clark were here to-night, he would recollect that once, when testing one of the Atlantic cables, there was a potential of about 100 cells in Newfoundland and little or no earth current in Valentia: that was a very remarkable fact, and not at all usual, I think.

The observations taken by me in 1858 were on record some years before Mr. Adams began his investigations, but they agree in a remarkable manner with his curves, both as to earth current and tidal curve.

In taking the observations of the thirty-six hours referred to, I happened to get into a very nice series of earth currents. The remainder of the observations are not so interesting, but I will plot them all out and give them to the Society at some future time.

The PRESIDENT: May I ask what the vertical curves are—cells or what?

Mr. SAUNDERS: They are merely deflections on a scale of division. I have data, and will reduce them to Daniell's cells.

The PRESIDENT: I am sure I shall be expressing the wishes of all present in saying that the Society accept very gladly Mr. Saunders' offer of the records of his observations.

I may point out that the curve of the 1858 observations, which Mr. Saunders has referred to, indicates a most remarkable correspondence between the tide curve and the earth current curve. These curves do not fall upon each other, but the periods are, so far as can be judged of from the actual unsmoothed curve, almost identical, one following some three hours behind the other.

Mr. WHIPPLE remarked that attention did not appear to have been called to the fact that magnetic variations, and probably also earth current variations, consisted of two classes of phenomena—the regular periodic movements and the storms or disturbances.

He believed that to General Sir E. Sabine belongs the credit of having separated the two in the observations recorded, and in his discussion of the colonial results this separation was fully carried out.

Possibly a similar method of dividing extraordinary from the more frequent earth currents phenomena would render the results of different observers more comparable. For instance, the failure experienced by Mr. Saunders to obtain satisfactory conclusions might possibly be attributed to the fact that disturbances were very frequent in 1870 and 1871, whilst in 1876 there were no perturbations worthy of remark.

He would like to ask the author of the paper whether in his discussion he had taken account of the fact that the magnetic pole of the earth was at a considerable distance from the terrestrial pole. This did not appear in the diagram, but would, in his opinion, produce a very considerable variation in the typical distribution of earth currents.

With regard to magnetic disturbances, the very abrupt manner in which they usually commenced induced the thought that they were due to some cause sudden in its action, possibly of the nature of a volcanic outbreak, rather than to the motion of a body moving in an orbit. Hence it would be unlikely to find a variation

corresponding to change of position of the moon in this class of phenomenon.

Mr. CHARLTON WOLLASTON: I had the opportunity so far back as 1846 or 1847 of watching some of the experiments which were at that time conducted by Mr. William Henry Barlow at Derby with reference to earth currents, and in 1851 availed myself of the opportunity of experimenting upon the submarine cable between England and France. The results I arrived at were such that I am very much disposed to think that the greater part of the so-called earth current is caused not by earth current at all, but by tidal current—the absolute mechanical movement of the water in the neighbourhood of the wires.

My experiment was this. Very soon after the first submarine cable was laid between England and France (a cable of four wires, only two of which were at the time in use), I was led to observe very strong deflections in the needles of the telegraph instruments, varying not in intensity only, but also in direction. I placed in circuit with one of the wires a delicate galvanometer, and obtained very remarkable results. The movements of the needle were so very strong that I thought there was some error attributable to having attached the "earth" wire to the water-pipes at Dover, and to a copper plate in France. I found the needle vary from about 40 degrees on the one side to 45 to 48 on the other, and that with a certain regularity which at the time I was not prepared to expect. A series of observations were taken for a fortnight every five minutes. These observations so entirely differed from those made by Mr. Barlow upon land lines radiating from Derby, that I was still of opinion I must have been somehow in error. Some time afterwards (I think in 1854) it occurred to me that the movements of the needle across the zero line, varying about 40 minutes in time daily, corresponded with the variation in time of the set of the tide up and down the Channel across the submarine cable.

I called upon the late Professor Faraday at the Royal Institution. Mr. Faraday (I recollect it as well as if it were to-day) said, "Oh, beautiful, beautiful," took down a book from the shelf, and read to the following effect:—"Supposing a wire could be suspended

from Shakespeare's Cliff to Cape Grisnez, the effect of the water running under that wire ought to be apparent," etc., etc. In fact, the observations taken between Dover and Calais coincided with effects such as Faraday had stated ought to be produced.

I have never had the opportunity of following the matter up, but the results were as I have stated; and I venture to think that the set of the tidal current of the river Severn will account for all the variations of the electric current observed by Mr. Adams between Cardiff and London. Experiments which I tried at East Greenwich at a later period quite confirmed those made upon the submarine cable. I found that, so long as the tide ran up the river, the needle was deflected in the one direction, and when running down the deflection was reversed.

I venture to think that the greater part of the variations in the curves shown in Mr. Adams' diagram are due to the tide of the Severn across the direction of the wires used by Mr. Adams in the course of his experiments.

Professor PERRY said: The great value of the paper we have heard lies in its being an exact record of continuous observations, and it is to be hoped that the complete curves will be published in the Journal. From the diagram for three days the lunar semi-diurnal periodicity is not very evident, but no doubt there are two positive and two negative maxima in a solar or lunar day. Very little good is to be derived, I am afraid, from theorising on the causes of earth currents until at every telegraph office a continuous record is kept, so that maps of equipotential lines may be drawn all over England. In a paper read in Japan in May, 1877, Professor Ayrton and I referred to Mr. Varley's predictions of bad weather derived from earth currents, and to the fact that earthquakes are known to be accompanied by greater intensities of these currents, and we designed a recording instrument which might be placed in any telegraph office, and which would neither interfere in any way with the ordinary working of the line, nor require any attention from the telegraph operator. At that time we wrote a paper on "Rain Clouds and Atmospheric Electricity," since published in the *Philosophical Magazine*, in which we referred to the results of certain elaborate calculations made by

us on electric tides and consequent earth currents, due to electrostatic induction from heavenly bodies. At the end of our paper "A New Theory of Terrestrial Magnetism," read before the Physical Society two years ago, we say: "In the preceding investigation we have supposed the electric charge to be uniformly distributed over the earth, and so have arrived at a law of magnetic intensity, varying merely with the latitude. But the sun and other members of the solar system may very likely have potentials so different from that of the earth, that we can hardly conceive the amounts; consequently, we should expect the static electric distribution of the earth would undergo periodic changes corresponding in time with those of the ocean tides. But alteration in the static distribution of electricity on the earth's surface means, as we have shown, alteration in the law of magnetic intensity; consequently, we should expect that this magnetic intensity would vary somewhat as do the ocean tides; and this is known to be the case. But it is also evident that, besides these regular changes, every time a great mass of vapour is suddenly formed and condensed on the earth, and whenever great changes are occurring in the solar atmosphere whereby the lines of electrostatic induction from the sun to the earth are altered, we should find corresponding changes in terrestrial magnetism such as we now know as magnetic storms. And not only this, but as the planets are charged bodies, their motions relatively to the sun ought to cause motions in the sun's atmosphere such that, for instance, the aligneation of a number of planets and the sun, or the near approach of any planets if the aligned or approached planets have potentials nearer that of the sun than many of the other bodies of the solar system, ought to diminish the storms in the solar envelope, and ought to alter the electrostatic distribution on the earth." Since that paper was published, it has been shown by Professor Rowland that the charge required on the earth to produce by its rotation alone the earth's magnetism, namely, five and a half microfarads per sq. c. m., is so great that this idea must be given up. I have not time now to consider whether or not it might not still be put in a workable form by assuming some other distribution than a merely superficial one;

but in its original form we have given it up, because the greatest superficial charge which we might reasonably be allowed for the earth will only suffice, by mere rotation, to account for the one-thousandth part of the earth's total magnetism. Even to produce this small effect, the outward pressure at the surface of the conducting earth would be as much as the one six-hundredth of an atmosphere. Possibly this is the greatest outward pressure which we are likely to be granted. If an outward pressure of one-sixth of an atmosphere were allowed, we could account on our theory for one-hundredth of the total magnetism of the earth. We relinquished this idea, then, on account of the great pressure outwards to which an exposed part of the surface of the earth would be subjected if the charge were great enough to produce the whole amount of terrestrial magnetism. We did not relinquish it on account of Professor Rowland's statement that "if the moon was electrified to a like potential the force of repulsion would be greater than the gravitation attraction of the earth, and it would fly off through space," because this is not the case. The mutual gravitation attraction of the earth and moon is about $1 \cdot 886 \times 10^{25}$ dynes, and the repulsion due to the charges which Professor Rowland assumed is $1 \cdot 401 \times 10^{24}$ dynes, or about fourteen times less. And in addition, we should never have proposed to give to the moon as high a potential as it is possible to give to the earth, since the greater the radius of the sphere the greater may be its potential for the same pressure outwards. The resultant attraction between the earth and moon is known, but who can positively say that it is altogether gravitation? The repulsion of charges of electricity on spheres very distant in comparison with their diameters will have exactly the same effect as a diminution of the mean densities of the spheres would produce on the total attraction, as it is very probable that the charges of all bodies of the solar system have the same sign.

As we informed the Physical Society a year ago, we do not propose to relinquish that part of our theory which deals with changes in the earth's magnetism, and with earth currents in telegraph lines. For if the density at any place changes by an amount equal to one twelve-thousandth of the charge which

Professor Rowland states that our theory requires, there will be produced at the place a change of nearly this fraction of the earth's total magnetic force (this is the amount required by actual magnetic observations), and such a change of density as this is quite allowable, since the pressure outwards due to it is less than the twelve-millionth part of an atmosphere. It is, however, somewhat difficult to see how this excess density is to be produced; for if the moon has a charge m, if f is the distance from earth to moon, if ψ is the angle made by the earth's radius at a place with the line of centres, then, whether or not the earth has an independent change, the fluctuating density at the place is very nearly

$$\sigma = \frac{3\,m}{4\,\pi\,f^2}\cos.\,\psi.$$

This shows that the moon's charge must be very considerable indeed to produce so large an electric tide. I have no time to pursue this subject further at present; but let me say that, whatever be our ideas as to the sufficiency of the theory to explain fluctuations in terrestrial magnetism, there can be no doubt of its sufficiency to explain earth currents.

It is obvious that Mr. Adams is wrong in giving to the earth on the side of the moon a charge of the same kind as that which exists on the side away from the moon. It has to be remembered that the ocean tide on the side away from the moon is due to the inertia of the water acted upon by the centrifugal force of the earth's rotation round centre of gravity of earth and moon. There is no analogy to this in the case of electricity. According to our theory there would only be one positive and one negative maximum in the strength of earth currents every twenty-four hours. He finds twice as many maxima, and we should be inclined to put this down to local circumstances.

It is probable that the oceans of the earth always possess the static changes necessary for their positions with respect to the moon. But over the more non-conducting land the necessary distribution cannot be effected quickly enough, and hence we get differences of potential which go under the name of earth currents. If this is true, then the coast-line of England is nearly an equi-

potential line, and the greatest earth currents will be obtained from the most inland place to the sea-coast. In India I understand that these currents are mainly in one direction. Mr. Adams finds from Cardiff to London two positive and two negative maxima in a lunar day, the differences of potential being exceedingly small. Other observers have found other results in other places; but as with all a periodicity is observed which is diurnal or semi-diurnal, it is probable that local causes will account for differences, just as differences in the periodicity of the ocean tides are due to local causes; the exciting agent in every case being static induction from the moon and sun.

In calculating the earth current effect, we find our smallest result, that is, no earth currents, when we consider the earth to be perfectly conducting. We therefore look at the problem in quite a different way from Mr. Adams. We find our greatest result when we consider a perfectly non-conducting earth. With a nearly non-conducting earth a comparatively small change on the moon will produce very considerable differences of potential. In this case, the earth's own charge would, in the course of time, have arrived at a stable state of distribution. The potential at a point, P, on the earth due to a charge, m, on the moon would evidently be

$$\frac{m}{\text{MP}}$$

M being the moon's centre. The electro-motive force in the direction of the steepest gradient (this direction changes with a diurnal periodicity) is

$$\frac{m}{f^2} \sin. \psi \text{ per centimetre,}$$

ψ being the angle made by the earth's radius at the place with the line of centres; so that during the lunar day we find the electro-motive force changing from $\frac{m}{f^2}$ to $-\frac{m}{f^2}$ per centimetre.

One volt per mile would be produced by the moon having the easily allowable charge of 10,000 farads. The greatest positive and negative earth currents would be found at places where the moon is rising or setting, whereas at lunar noon and

midnight there would be no earth currents. I have not troubled you with an account of the much more complicated problem which was taken up by Professor Ayrton and myself, the earth being what it is, a moderately good conductor. The result is somewhere between the results for the two limiting cases which I have given. It is then evident that the sun or moon's induction is quite sufficient to account for very considerable earth currents, whatever may be our difficulties in the co-ordination of the phenomena of terrestrial magnetism. It is, however, evident that we must consider local circumstances as modifying the phenomena.

With reference to the connection between earthquakes and these phenomena, there seems to be no doubt that the intensity of the earthquakes, continuously producing the earth crackling which is heard from the buried microphones of my friend Professor Milne, follows a lunar periodicity. This earth crackling is undoubtedly due to elastic tides.

Mr. C. F. VARLEY: If you will allow me, I should like to say just one word. I should like to ask Mr. Adams if he could before the next meeting (when the discussion will be continued) give us the resistance of the circuits. In one place he speaks of a current of so many Daniell cells per mile, and in another place he speaks of a current of so many milliwebers. If he would give us the resistance of the circuits and apparatus between the two termini, we should be able to compare them. I would also like him to give us the actual figures from which he has drawn his various curves.

A ballot for new members then took place, and the meeting adjourned until February the 24th, 1881.

Foreign Members:

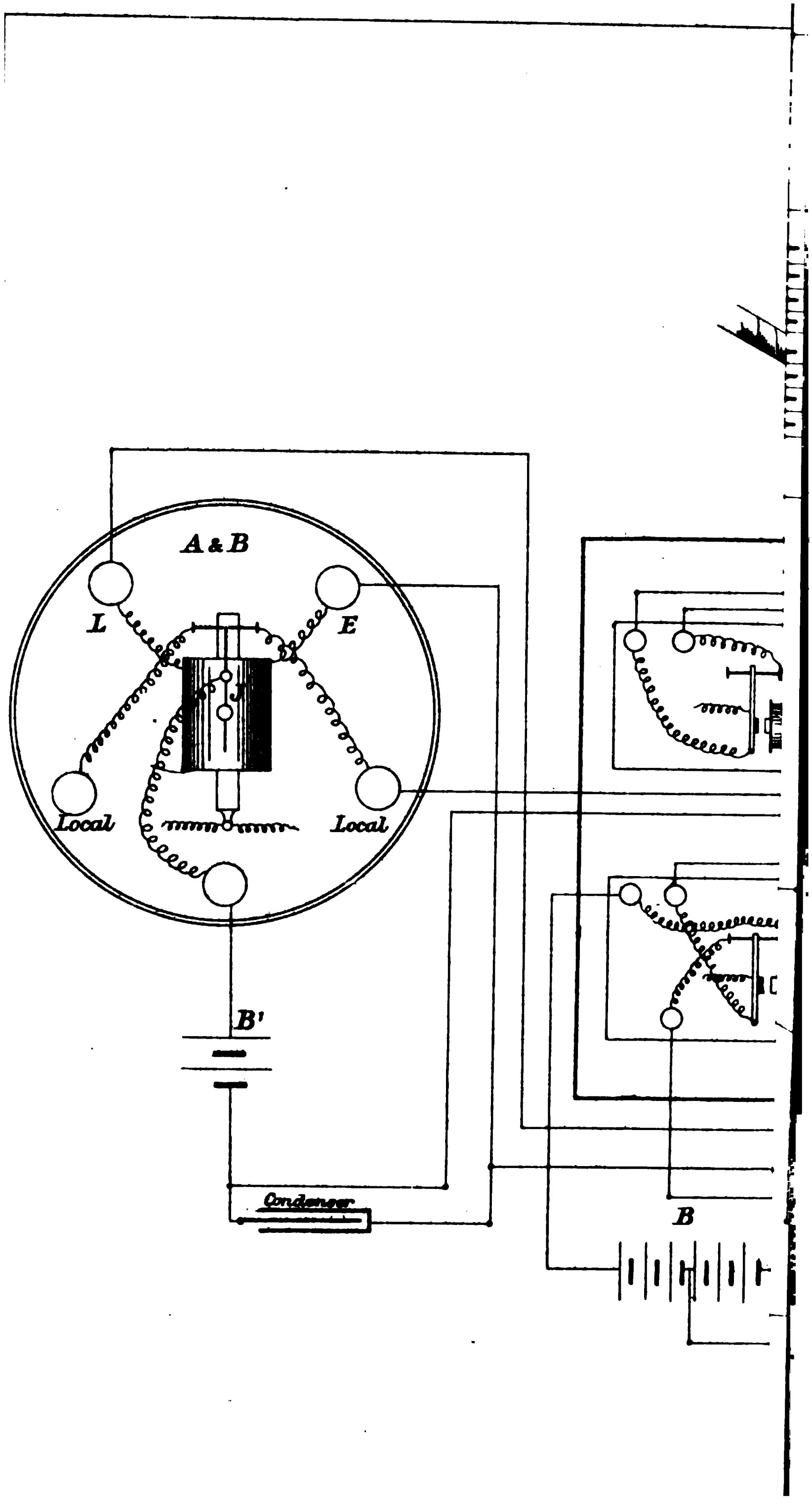
Joseph Banneux.
John A. de Braam.

Members:

William Crookes, F.R.S.
Robert Lecon John Ellery.
Robert Campbell Laughlin.
Grainger Lawrance Towers.

Associates:

Percy Allan.
Peter Robert Challen.
Hugh Erat Harrison.
J. Orchiston.
George Palgrave Simpson.
Charles Edward Walduck.
E. Cox Walker.


A & B
L
E
J
Local
Local
B'
Condenser
B

ORIGINAL COMMUNICATIONS.

MANUAL TRANSLATOR.

By T. J. WILMOT, Associate.

his plan combines the closed-circuit system on the land line he double-current system on the cable. The apparatus con- of one Allen & Brown's submarine relay, two American arised relays (one of which has the back stop utilised), one ican key, one switch, one circuit-closer.

he *modus operandi* is as follows, and will be understood upon ence to the diagram:—The switch being turned to "send," ine current from the distant land station actuates relay R^2, ng to earth through the circuit-closer K. This brings ry B into operation, sending double current signals through witch into the cable. Upon turning to "receive," the switch r lifts that of the circuit-closer, and opens it at point Q (Fig. ringing the armature of relay R^1 into circuit; signals from the e from the switch through the coils of the submarine relay A B to earth. A local battery is thus brought into play, operating y R^1, the armature of which passes the signals into the land The key is inserted in the land line, and thus speaks to land ons direct, whilst it works relay R^2 for speaking through the e. The condenser is for the purpose of preventing the spark he contact points in the submarine relay A and B.

Fig. 1 shows the switch *at rest.* In that position either side speak. If cable commences speaking, the switch lever is ed to its farthest point on the "receive" side, thus lifting the ite lever of the circuit-closer, which opens the line to the ature of relay R^1, as explained above. I may add that the ch and circuit-closer are of my own design and manufacture. latter is made in the form indicated, so as to keep the land

line and cable as far apart as possible, and so preventing any lightning discharge passing.

NOTE.—We are now using a D.C. Transmitter, of the form employed by the Western Union in their Quadruplex, for passing the signals into the cable. It works well, but hardly as well as the simple relay, as shown in the diagram.

AUTOMATIC DOUBLE CURRENT TRANSLATOR.

The apparatus required for this are as follows:—

1 Allen & Brown's submarine relay, A and B.

1 Siemens' polarised relay (doubly wound), R^2.

2 American unpolarised relays (1 with back stop utilised, other doubly wound), R and R^1.

1 Electro-magnetic switch, S.

2 Galvanoscopes.

1 S.C. Key.

1 Compound switch, S^1.

2 Resistances (one for balancing line, other smaller one for shunt).

The switch S is upon the same principle as the one used for the "Manual," with the exception that it is turned by means of a pair of coils; and it is arranged so as to be controlled entirely by the land station.

Signals from the cable pass first through the galvanoscope to switch, and from thence through A and B to earth. A and B operates R, which sends D.C. signals to line, which pass through R^1 and R^2 without moving them.

When the land station sends, his signals pass from the galvanoscope through R^2 and R^1, R^1 is closed, which in its turn closes S, and carries the switch over to "send." R^1, being unpolarised, remains closed the whole time the land station continues sending, whilst R^2 sends D.C. signals into the cable from battery B^1.

If the translator station wishes to speak, he turns switch S. This diverts battery B from R to the key. The key is placed in

the land line, so that all stations can see his signals, and as the whole power of battery B would pass through R^2, a small shunt is provided which may be adjusted to give the requisite strength of signal from R^2 into the cable.

As the relay A and B is unstable, it is necessary to leave battery B in circuit; but no matter which may be to line at the moment, the marking or the clearing current, the moment the land station turns his switch to "send," battery B is completely cut off, and the line put to earth through the armature of S.

The condenser, or a high resistance, is needed with A and B for preventing the spark, as in the "Manual."

The instruments are provided with efficient lightning guards.

ABSTRACTS.

W. SIEMENS—THE VARIATION OF THE CONDUCTIVITY OF CARBON WITH TEMPERATURE.

(*Annalen der Physik und Chemie, B. X., H. 4, No.* 87, 1880, *pp.* 560-574.)

Matthiessen was the first to observe the peculiarity that the conductivity of carbon increases with the temperature. Beetz found that this law did not hold good for gas-retort carbon, but only for artificial carbon made of powdered carbon stuck together with a solution of sugar, then pressed and heated until the sugar was charred. Lately, Felix Auerbach found that the resistance of carbon increased with the temperature.

Dr. Siemens made cylindrical carbon rods of different thicknesses and lengths; the ends were electrotyped with copper, and leading wires attached. For low temperatures the carbon was heated in a bath of melted paraffin, and for high temperatures the carbon rod was put in a copper tube, the ends of which were closed with plaster of Paris plugs, and which was placed in an open fire-brick oven. Raising the temperature caused the resistance to diminish from 1·452 at 20° to 1·375 at the temperature of molten tin, and 1·298 at the temperature of molten zinc. Cooling the tube rapidly, however, caused the resistance to increase to 1·685 when the temperature had cooled down to 20° C. This increase the author attributes to actual burning away of the carbon rod. The discrepancy between the results of Matthiessen and Auerbach, Dr. Siemens considers, is due to the latter not having electrotyped his carbon rods, but only having dipped them in melting solder, and in consequence of which there was a layer of gas originally occluded in the carbon, between the carbon and the connecting wire. And, as a proof of this, he found experimentally that when Auerbach's method of dipping the ends of the carbon rods in molten solder was adopted, the resistance increased with temperature, whereas when Matthiessen's or his own method of electrotyping the carbon was employed, the resistance diminished with elevation of temperature. The paper concludes with experiments on Berlin gas-retort carbon and artificially made carbon, both of which were found to follow Matthiessen's law.

FELIX AUERBACH—MAGNETIC INVESTIGATIONS.

(*Annalen der Physik und Chemie, B. XI., H.* 3, *No.* 11, 1880, *pp.* 353-394.)

The author has endeavoured to determine what influence variation of mass, length, and thickness, and density of a magnet has on its temporary magnetism, other things remaining the same. Investigations on the influence of length and thickness have been made by Lenx, Jacobi, and Dub. From these it follows

that the magnetism $m \propto l^k$ where k varies between 2 and $\frac{3}{2}$, when the thickness is 1·75 inches. The author found that in most cases $m \propto l^2$, or better $m \propto l^{2+k}$, where k was between 0 and $\frac{1}{2}$. These formulæ, however, do not give the actual effect of the length, since the mass is proportionate to the length. We must, therefore, divide by l, and we find that $\mu \propto l^{1+k}$. Quite analogous is the influence of the diameter. The author found that $m \propto d^{\frac{1}{2}}$, or, better, $m \propto d^{1-k'}$ where k' was between 0 and $\frac{1}{2}$. Dividing by d^2, as the mass is proportional to d^2, we have $\mu \propto d^{(1+k')}$. The temporary magnetism increases, *cæteris paribus*, with the length and diminishes with the diameter, and assuming that $k' = k$, which is very near the truth, we have $\mu \propto \left(\frac{l}{d}\right)^{1+k}$ in which k is between 0 and $\frac{1}{2}$. Considering the magnetised body to consist of molecular magnets, which in addition to external forces are subject to mutual influence, we must conclude that parts near the surface behave differently from those inside. Using a mixture of clean iron powder with fine sawdust put into a glass tube, and surrounded with the magnetising spiral, the author found that the temporary magnetism of the powder increased with its density faster for small and great densities than for those intermediate. Regarding E. Becquerel's assumption that the difference between the magnetism of iron, nickel, and cobalt is only a result of the different densities of these metals, the author made experiments with nickel powder similar to these referred to above. He found that, *cæteris paribus*, the temporary magnetism of nickel powder increases with the density. For excessively small densities the nickel powder had more than half the temporary magnetism of iron powder of the same density, for small densities it had just half as much, with increasing densities the ratio decreased, and at half the natural density it was a quarter. Regarding the influence of the magnetising force on the temporary magnetism, he found that with both solid and powdered metal for small forces, the magnetism was proportional to the force, for greater ones it increased faster, and for still greater it increased more slowly again. The latter portion of the paper contains a theoretical explanation of these phenomena.

C. BAUER.—NEW INVESTIGATIONS ON MAGNETISM.

(*Annalen der Physik und Chemie, B. XI., H. 3., No.* 11, 1880, *pp.* 394-413.)

If s is the value of the magnetising force in a homogeneous magnetic field, v the volume of a body brought into it, and m the corresponding induced magnetic moment, $m = k\,v\,s$ where k is the coefficient of magnetisation. For determining k the author used an iron ring of circular section coiled with wire, through which a current was passed, and the amount of magnetism was measured by observing with a galvanometer the current induced in a secondary coil wound also on the ring. In such a case

$$s = \frac{2\,m_1\,\mathrm{I}\,\mathrm{L}}{\pi\,\gamma^2}$$

where m_1 is the number of convolutions of the primary, i the strength of the

current, γ the radius of the section of the iron ring, $L = 2\pi(\rho_0 - \sqrt{\rho_0^2 - \gamma^2})$, ρ_0 being the radius of the circular axis of the ring. And

$$k = \frac{w}{16\, m_1\, m_2\, \pi\, L} : \frac{j}{I} - \frac{L_1}{4\pi L}$$

where w is the resistance of the secondary circuit containing m_2 convolutions, and in which a current whose integral strength, j, is induced, $L_1 = \int \frac{d f}{\rho}$ the integration to extend over the the section of the secondary circuit.

The author found that $k = 15 + 10x$, and $m = 15x + 10x^2$. To observe the effect of change of temperature, the ring was hung in an iron vessel with double walls, and in which the ring could be heated to a temperature of 150° C., and kept constant for hours. The general conclusions arrived at were:—

1. The influence of temperature on the magnitude of k depends on the magnetising force.

2. If the magnetising force is constant, k increases as the temperature increases, this law holding good up to a certain value of the magnetising force. For greater values k diminishes as the temperature is increased.

3. The smaller the magnetising force is, the greater is the influence of temperature on the value of k.

The author finds that if k_2 corresponds with the temperature t_2 and k_1 with t_1, and if $t = t_1 - t_2$ then $k_2 = k_1(1 + q_1 t + q_2 t^2)$ q_1 and q_2 being determined by the method of least squares; q_1 was found to decrease with increasing x, was nought for $x = 36$, and then became negative; for $x = 62$ it was a minimum and afterwards increased; q_2 was always constant. Next the author determined the connection between q_1, q_2, and x, and eventually he arrived at the result that

$$k_2 = k_1\left(1 + \frac{a - bx}{x}\, t + \frac{c}{x}\, t^2\right)$$

where $a = 0{\cdot}005685$; $b = 0{\cdot}0001122$; $c = 0{\cdot}0000072$.

The following are the results obtained on experiments made on an iron rod 300 millimetres long and 8 millimetres thick, which was first heated to a red heat, then put into the magnetising spiral and allowed to cool:—

For small magnetising forces the temporary magnetic moment increases first with increasing temperature, reaches a maximum at a red heat, and then suddenly falls to nought.

For greater magnetising forces the temporary magnetic moment decreases gradually as the temperature increases, and falls suddenly at red heat to a very small value. A large number of experiments were made on the first appearance of the temporary magnetism at a white heat, and it was found to be stronger for great magnetising forces than for small ones. G. Gore having observed that when an iron wire is heated it becomes suddenly elongated at a fair red heat, and when cooled it contracts suddenly at the same temperature; also that the rod suddenly loses a part of its temporary magnetism which it suddenly regains at the same temperature on cooling, Mr. Bauer made experiments to determine how far the phenomenon was dependent on the value of the magnetising force,

and he found that Gore's phenomenon became more marked, lasted for a longer time, and occurred at a higher temperature the greater was the magnetising force—that is, for small magnetising forces the phenomenon commenced at a bright heat and ended at a dull red, whereas for great magnetising forces it continued from a very white heat until the rod had become quite dark.

The remainder of the paper deals with experiments on the coefficient of magnetisation of iron wires, iron rods, and of electrolytic iron prepared by passing a weak current through a solution of iron sulphate, and it was found that the maximum value of the coefficient was very quickly reached in ordinary iron, a little later in iron filings, and latest of all in electrolytic iron.

J. STEPHAN—THE LIFTING POWER OF MAGNETS.

(*Proceedings of the Academy of Science, B. LXXXI; Part II., January,* 1880.)

In considerations of lifting power of magnets, three kinds of force must be distinguished, viz.: 1, the attraction of the oppositely magnetised portions in contact; 2, the attraction of the free magnetism of the magnet on the portion of the armature in contact; 3, the attraction of the free magnetisms in the magnet and in the armature on one another.

The author discusses a ring magnet magnetised by a spiral coiled round it, and through which a current passes, and he shows that, if one-half of this ring be regarded as magnet and the other half as armature, the attractive force is $2 \pi \mu^2 q$ where μ is the magnetic moment per unit of volume, and q the area of the surface in contact. This formula is only applicable when, 1, neither magnet nor armature contain any free magnetism; 2, the lines of force are at right angles to the surfaces in contact; and, 3, the intensity of magnetisation is the same at all points of the surfaces in contact. M. Stephan considers experiments of Rowland's on the maximum value of the coefficient of magnetisation and Joule's experiments on the actual lifting power of magnets, the former having arrived at the conclusion that a lifting power of 177 lbs. per square inch would be the maximum attainable, while the latter had obtained experimentally 175 lbs. The author criticises the statement in Jenkin's "Electricity" that Joule obtained 200 lbs. per square inch, since he considers Joule only regarded 200 lbs. per square inch as a possible attainable maximum. Very few experiments have been made with electro-magnets sufficiently symmetrical to allow of exact calculations being made, those of v. Waltenhofen, however, with ring magnets being, to a certain extent, an exception, and in these a maximum force of 240 lbs. per square inch was obtained, but even this number can only be regarded as approximate. Rowland's experiments are next considered, and the author shows the importance of drawing a curve having for abscissæ the magnetic moment per unit volume, and for ordinates the ratio of this magnetic moment to the magnetising force, and which ratio quickly reaches a maximum, then falls and approximates to a straight line, cutting the axis of x at a small angle.

If the plane cutting the ring be not at right angles to the lines of force,

the lifting power will be less than before, even although the surfaces in contact are larger in area; and the author shows that this lifting power is equal to $2 \pi \mu^2 \mu q \sin. e$, where e is the angle between the lines of force in the ring and the normal to the section; and when the ring is divided into two parts, the lengths of which are as 5 to 1, the lifting power is shown to be only half of that obtained when the ring is divided into two equal portions. The author next considers the change produced by the lines of force in different parts of the section not meeting the plane of the section at the same angle, and afterwards the alteration arising from the intensity of magnetisation being different in the interior parts of the ring from what it is near the surface.

The case of the magnet and its armature forming a ball is next entered into. In a ball magnetised by a current flowing in a coil of wire wound round it, the magnetism is uniform throughout, and there is no free magnetism except on the surface. If, by a plane at right angles to the magnetic axis, the ball be divided into magnet and armature, the lifting power due to the magnetism at the surfaces of contact will be $2 \pi \mu^2 q$, where μ is the magnetic moment per unit volume, and q the area of the surfaces in contact. In addition to this, however, must be considered the actions of the free magnetism on the magnet, on the free magnetism on the armature, and on the magnetism on the contact surface of the armature, and the amount of this subtraction is proved to be equal to $\pi \mu^2 q (\frac{1}{3} - \cos.^2 \alpha) - \frac{4}{3} \pi \mu^2 q$ where α is the angle between the magnetic axis and the radius drawn to the edge of the contact surface. Hence the total lifting power of the magnet is equal to $\pi^2 \mu^2 a^2 \sin.^4 \alpha$ where a is the radius of the ball. This expression has a maximum when $\alpha = 0$, that is, when the magnet and the armature are each half the ball.

H. HERWIG—ON THE INFLUENCE OF TEMPERATURE ON THE PHENOMENA OF CHARGE OF A FLUID CELL ACTING AS A CONDENSER.

(*Annalen der Physik und Chemie, B. XI., H. 4, No. 12, pp.* 661-686.)

It had been known that in a liquid cell with platinum electrodes, the liquid being water, the polarisation produced by electrolysing currents decreases with an increase of temperature. This was explained on the hypothesis that at a higher temperature the gaseous products of decomposition were farther removed from the electrodes. Exner considered that a smaller quantity of peroxide of hydrogen was formed. The author made a long series of experiments on the subject, in all 156, the results of which are given in numerous tables. He followed the example of Beetz in experimenting with small electro-motive forces, varying from 0·474 volts to 0·88 volts. The liquid cell was supported in such manner that it could be surrounded by a bath which could be heated to any desired temperature, without being in any way disturbed. The temperature of the cell was read off on a thermometer immersed in it. The electrodes were very long and thick polished plates of platinum, which only dipped partially into the liquid in the cell; the area of the immersed portion was

carefully determined in each experiment. The liquid used was water slightly acidulated with sulphuric acid, the resistance of which was extremely small in comparison with that of the wire completing the circuit of the cell (19,100 ohms). The values obtained show that a very much stronger electrolytic convection takes place at higher temperatures than at lower, a result which has been also stated by Helmholtz. To compare the experiments made at different temperatures, the discharge currents were observed, after a long-continued charging, which got rid of the first fluctuations of the convection resistance. The discharge was observed during one minute, the minute being divided into four equal parts; the first interval of fifteen seconds was sub-divided at the instant when the needle of the galvanometer completed its first deflection α. At this instant the current strength is given by the formula

$$\phi_1 = \frac{\alpha}{1 + e^{-\lambda}}$$

where λ is the damping. If t_1 is the time of the first deflection, then, as shown in a former paper, if ϕ_1 is the original intensity

$$\frac{c}{\frac{1}{R} + \frac{1}{\omega}} = \frac{t_1 \log. e}{\log. \phi_0 - \log. \phi_1}$$

If ϕ_2 is the intensity after fifteen seconds, then

$$\frac{c_1}{\frac{1}{R} + \frac{1}{\omega}} = \frac{(15 - t_1) \log. e}{\log. \phi_1 - \log. \phi_2}$$

The current strength is then calculated from the formulæ

$$\int_0^{t_1} i\, d\, t = c\, E \left(\frac{\omega}{R + \omega}\right)^2 \left\{1 - e^{-\frac{t_1}{c}\left(\frac{1}{R} + \frac{1}{\omega}\right)}\right\}$$

and

$$\int_{t_1}^{15} i\, d\, t = c_1\, E \left(\frac{\omega}{R + \omega}\right)^2 \left\{e^{-\frac{t_1}{c_1}\left(\frac{1}{R} + \frac{1}{\omega}\right)} - e^{-\frac{15}{c_1}\left(\frac{1}{R} + \frac{1}{\omega}\right)}\right\}$$

The sum of these expressions gives very nearly the current strength during the first 15 seconds of the discharge. The quantity of electricity for the following 45 seconds is obtained from the formula

$$15 \left(\frac{\phi_2}{2} + \phi_3 + \phi_4 + \frac{\phi_5}{2}\right)$$

where ϕ_3, ϕ_4, ϕ_5 are the values of the current strength at 30, 45, and 60 seconds, respectively, after the commencement of the discharge. The quantity of charge, other than that shown by the return current, was found from the formula

$$(c\, Ⓜ)_0^{t_1} = \frac{R + w}{w} \int_0^{t_1} i\, d\, t, \text{ etc.}$$

In each experiment, besides the temperature, the other factors were varied,

viz., the electro-motive force used for charging the cell, the size of the electrodes, the degree of concentration of the liquid, and consequently its conductivity, and the direction of the charging current. The following are the most important results obtained:—

1. The convection resistances decrease markedly with increasing temperature, and more with small electro-motive forces than with large.

2. According to the quantity of the convection resistances, the relations of the quantities of electricity really flowing back in the discharge currents at increasing temperatures may prove to be quite different.

3. If, besides the quantities of electricity really flowing back in the discharge currents, those which flow out of the condenser simultaneously during the discharge in the direction of the charging current, owing to the convection process, be taken into account, higher values are obtained for the quantities of charge (c Q) under all circumstances at higher temperatures. If the convection resistance is infinite, the charge and the current intensity coincide. Hence in a case not disturbed by electrolytic convection, the charge as well as the discharge current would be found much greater at higher temperatures.

4. The influence of temperature is decidedly stronger for smaller electro-motive forces than for larger.

5. The course of the discharge is always decidedly slower at higher temperatures. The better rearrangement of the molecules at higher temperatures occurs therefore at first quite gradually.

6. The above relation holds for smaller electro-motive forces in a higher degree than for larger.

7. Under otherwise similar circumstances the stronger electro-motive forces require throughout at lower temperatures a slower course of discharge; at higher temperatures, on the contrary, just as quick, sometimes even a quicker course than the weaker forces.

8. Greater or less concentration of the liquid has no perceptible influence on the phenomena.

9. Larger surfaces of the electrodes require always a slower course of discharge.

In order further to investigate the question, the author made use of a glass tube 1 cm. in diameter, bent five times, and filled with very dilute sulphuric acid; in the open upper ends of the tube two small platinum plates were placed to serve as electrodes. The intermediate portion of the tube was immersed in a cold bath, so that only that portion of the liquid in the neighbourhood of the electrodes became warmed. In this case the influence of the temperature was predominant near the electrodes, and did not occur in the intermediate liquid. Besides, a distinct diminution of the convection resistance was quite unmistakable, especially in the intermediate liquid. Since the resistance of the liquid was extremely small in comparison with the wire resistance of the circuit, this last relation could not be explained by what occurred merely at the electrodes.

The author next limited his observations to the two electrodes. The thick platinum plates used in the first experiments were immersed separately in two beakers, which were connected by a siphon, each beaker being surrounded by a bath. The liquid resistance was again small in comparison with that of the rest of the circuit (19,100 ohms). From these observations he concluded—

10. The convection resistance is diminished in a great degree, if there is a high temperature at only one electrode. Moreover, the heating of the anode has decidedly more effect.

11. The charges are increased if the kathode is heated.

12. With the increased action of the heated kathode at the commencement of a series of experiments, is connected a slower course of discharge, as compared with the heating of the anode.

13. The observation in No. 11 could not be made with the strongest electromotive force used (0.88 volts).

14. In cases where the differences of charge for heated anode or kathode are great, the charges obtained when both electrodes are heated together approximate to the charges obtained when the kathode alone is heated. If the differences for the two electrodes are small, the charges, when both are heated simultaneously, are decidedly greater. The author concludes that the most important results obtained may be explained by the greater freedom with which the gas collected at the one electrode returns through the liquid to the other, if the electrodes are heated, thereby rendering new convection currents possible. On the other hand, a larger charge is obtained by heating the kathode. The increased mobility of the electrolytic molecules at higher temperatures explain the increase of charge. The electricity collecting at the electrodes is in a position to bring about more easily the rearrangement of the molecules, and consequently increases the capacity of the cell. The rearrangement of the molecules proceeds most powerfully from the kathode, *i.e.*, from the electrode most free from gas, which can easily be understood, since here the charged surface of the platinum plate is in immediate contact with the molecules of the liquid. The gas which has been absorbed by the electrodes is set free more easily at a higher temperature, and also assists the above cause, since the setting free of the gas renders possible a stronger convection current. If the conclusions as to the parts played by each electrode are right, then these functions will be reverted if the gas essentially concerned is hydrogen in place of oxygen. To test this, both electrodes were first covered with a film of hydrogen in another cell. On the experiment being performed, the anticipated result was obtained. The author also made some experiments on the production of thermo-electric currents between hot and cold platinum plates immersed in dilute sulphuric acid. With an absorption of oxygen, the current in the liquid was from the hot to the cold platinum, and the converse when the hydrogen was absorbed. The stronger convection currents in the former experiments follow the same course.

W. HOLTZ—ON THE INCREASE OF DANGER FROM LIGHTNING, AND ITS PROBABLE CAUSES.

(*Annalen der Physik und Chemie, B. XI., H. 4, No. 12, pp.* 719-722.)

The author refers to a comparison made by Von Bezold, in 1869, between the increasing frequency of thunderstorms and the increasing frequency of lightning strokes in Bavaria, from which the latter concluded that the one was the cause of the other. The author has investigated whether the increase of lightning strokes in recent times holds good generally, and, further, if the causes are to be sought for in meteorological, or rather in terrestrial changes. The data were taken from the records of the meteorological stations, and of the insurance offices, and are set out in the following table, in which danger from lightning is taken as the quotient of the number of houses struck, and the total number of houses in the district:—

District.	Decrease or Increase of Thunder Storms				Decrease or Increase of Danger from Lightning			
	Since 1854.	Since 1862.	Since 1870.	Number of Places.	Since 1854.	Since 1862.	Since 1870.	Number of Places.
West Germany ...	1·15	1·35	1·05	20	2·64	2·51	1·05	11
East ,, ...	0·97	1·15	0·88	15	2·86	2·69	1·45	5
North ,, ...	1·1	1·31	0·97	23	2·67	2·84	1·26	8
South ,, ...	1·04	1·21	1	12	2·85	2·11	0·99	8
Germany (generally)	1·07	1·27	0·98	35	2·75	2·57	1·12	16
Austria	0·88	0·79	0·97	7	1·75	1·24	1·06	2
Switzerland	...	1	1·03	2	2·07	1·83	1·12	4

From the first and last mean of four years the first mean = 1.

These tables show that the increase of thunderstorms is only extremely small, and changes to a decrease; while the increase of danger from lightning is surprisingly great, and in no instance changes to a decrease. The author concludes that the increase in danger from lightning is to be sought for in terrestrial changes. Among these may be placed the increase in cleared forest land, and perhaps the increase of railroads, since both these tend to attract thunderstorms to towns and villages; also the increase, in recent years, of the quantity of metallic material used in building, as in ornamental roof work and in gas and water-piping.

H. PELLAT—EXPERIMENTS ON THE DIFFERENCE OF POTENTIAL OF TWO METALS IN CONTACT.

(*Journal de Physique, T. X., No.* 110, *February* 1881, *pp.* 68-76.)

The author used his apparatus described in vol. IX., p. 145 of the *Journal de Physique*, and abstracted in Vol. IX., No. 33, p. 326, of this Journal, to determine how the apparent difference of potential depended on the superficial

layer of substances opposed to one another. A plate of copper was chemically cleaned, and found to have electro-motive force of contact with gold equal to 0·187 Latimer Clark's cells. The copper plate was placed for an instant in sulphuretted hydrogen gas and then washed again in alcohol, but the electro-motive force of contact with gold had gone up to 0·201. Experiments were next made with a plate of zinc with gold, and it was found that the electro-motive force of contact, which was 0·698 just after the zinc had been most carefully cleaned, diminished to 0·523 in a quarter of an hour, the plates being in the air and no visible alteration having occurred at the surface of the zinc. The effect of rubbing the surface of the zinc in different ways was next tried, and the author arrived at the result that all metals became more positive when the surface was rubbed; that this effect died away slowly if the scratching had been deep when emery paper was used, and more quickly if the rubbing had been less deep, filtering paper being only employed in the cleaning. Zinc showed the greatest change with the mode of cleaning the surface, and copper the least.

Experiments were next made on the effect produced by altering the pressure of a gas surrounding the plates, and which was chemically inactive; the results arrived at were that the difference of potential increased with diminution of potential, and regained its original value when the pressure was again increased, the greatest change in any case obtained being not more than $\frac{1}{35}$ of the total difference of potential. A curve is given showing the variation of difference of potential with variation of pressure when the gas surrounding both plates was oxygen. The author found that the substitution of one gas for another produced little change when the pressure was small. The author next investigated whether two metals joined by a liquid arc were at the same potential, and he considered it important in this experiment that the surfaces opposed to one another and acting inductively on one another should be those that are joined by the liquid. As, however, the difference of potential changes with time, and as it was the first difference of potential he desired to measure, the time law of change was determined by several experiments, and thus the initial difference of potential arrived at by interpolation. The conclusion arrived at was that joining two metals by alcohol reduced at the first moment the surfaces touched by the alcohol absolutely to the same potential, a conclusion that can be usefully employed in determining the difference of potentials of rare metals that cannot be obtained in plates.

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NAYLOR, J. E.	Postal Telegraphs, Leeds.
NEEDHAM, R. J.	Anglo-American Telegraph Company, Duxbury, Mass., U.S.
NEVETT, F. W.	Direct United States Cable Company, Torbay, N.S.
NEWBURY, GEORGE C.	Postal Telegraphs, General Post Office, E.C.
NEWMAN, A. E.	Compañia Telegrafica del Rio de la Plata, Monte Video.
NEWNHAM, W. A.	Indo-European Government Telegraphs Jask, Persian Gulf.
NICOLSON, JAMES	Compañia Telegrafica del Rio de la Plata, Monte Video.
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OAKSHOTT, A.	Postal Telegraphs, Southampton.
OATWAY, ARTHUR	Postal Telegraphs, General Post Office, E.C.
O'DONNELL, C. E.	The Indian Government Telegraph Department.
OGG, F. WILLIAM	11, Barclay Terrace, Barclay Road, Leytonstone, Essex.
O'HAIRE, J. M.	Government Telegraphs, Cape of Good Hope.
ORCHISTON, J.	Telegraph Department, Auckland, New Zealand.
PAGE, ALFRED S.	India Rubber Company, Silvertown.
PAGE, CHARLES J.	Enterprise Estate, Deli-Penang.
PARMITER, ALBERT	Postal Telegraphs, Gloucester.
PARSONE, E. W.	175, Adelaide Road, N.W.
PARSONS, The Hon. RICHARD	10, Connaught Place, W.
PAUL, A. J. E.	Eastern Telegraph Company, Bona.
PAUL, A. L.	16, Kensington Square, W.
PATEY, CHARLES H. B.	The Cedars, Bickley.
PATTEN, FREDERICK A.	Care of Messrs. Grindlay & Co., 55, Parliament Street, S.W.

Payn, John J.	Eastern Telegraph Company, Marseilles.
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Perkins, E. J.	
Penrose, Cooper, Lieut., R.E.	The Horse Guards, Whitehall, S.W.
Perry, F.	Anglo-American Telegraph Company, Heart's Content, Newfoundland.
Peters, Alfred	Western and Brazilian Telegraph Company, Bahia.
Petersen, K.	German Union Telegraph Company, 4, Werder Strasse, Berlin.
Petterson, Oscar F.	Direct United States Cable Company, Torbay, Nova Scotia.
Philipps, W. Rees	Care of H. Rees Philipps, Esq., M.A., India Office, S.W.
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Portelli, Robert	Eastern Telegraph Company, Malta.
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Price, H. N. Blackwood	Eastern Telegraph Company, Candia, Crête.
Probert, J.	Postal Telegraphs, Moorgate Street Buildings, E.C.
Puget, Colonel Granville W.	34th Regiment, Ferozepore, India.
Pyne, Stree Bollychand	Indian Government Telegraphs, Calcutta.
Raymond, George	Eastern Telegraph Company, Tenedos, Turkey.
Reynell, C. H.	Brazilian Submarine Telegraph Company, Madeira.
Rich, H. R.	Indian Government Telegraphs, Calcutta.
Richards, A., Sergeant R.E.	Canterbury.
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Richardson, H.	Postal Telegraphs, Douglas, Isle of Man.
Riemenschneider, Fredk. W. C.	Great Northern Telegraph Company, Libau, Russia.
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Roberts, Martin F., F.C.S.	Postal Telegraphs, Gloucester Road, N.W.
Rogers, F. M.	29, Moorgate Station Buildings, E.C.

ROLLS, EDWARD T. . . .	L. & S. W. Railway, Exeter.
ROPE, L. W.	Anglo-American Telegraph Company, Brest.
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ROSS, WALTER O. . . .	West India and Panama Telegraph Company, St. Thomas, West Indies.
ROWE, THOMAS	Pointsman Inn, Whitelands Road, Stalybridge,
RUSHTON, WILLIAM S. . . .	General Post Office, E.C.
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SCHAEFER, LOUIS . . .	Eastern Telegraph Company, Malta.
SCOTLAND, JAMES . . .	Anglo-American Telegraph Company, St. Pierre, Newfoundland.
SCOTT, J. H.	Nutsford Vale Rope and Wire Works, West Gorton, near Manchester.
SENNETT, ALFRED R. . . .	Seacroft, West Worthing.
SHAW, Major W.	
SHEPHERD, F.	Postal Telegraphs, Moorgate Street Buildings, Telegraph Street, E.C.
SHIELD, CLIFTON . . .	Reform Club, Pall Mall, S.W.
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SLATER, WILLIAM . . .	Western and Brazilian Telegraph Company, Pará, Brazil.
SMIBERT, GEORGE . . .	Telegraph Department, Melbourne.
SMITH, A.	Indo-European Government Telegraphs, Persia.
SMITH, F. J.	The Crescent, Taunton.
SMITH, JOHN	Inspector of Telegraphs, Cape Town.
SMITH, FREDERICK . . .	Caledonia Iron Works, Halifax, Yorkshire.
SMITH, JOSEPH STANLEY, C.E. . .	Homefields, Shanklin, Isle of Wight.

Smith, A. Toulmin . . .	Selborne Chambers, Chancery Lane.
Smythe, John . . .	Valentia, Ireland.
Spratt, G. O. . . .	Eastern Telegraph Company, Porthcurno, Penzance.
Squier, J. E. . . .	Eastern Extension Telegraph Company, Hong Kong.
Squires, Frederick . .	Anglo-American Telegraph Company, Brest.
Squires, George . .	Anglo-American Telegraph Company, St. Pierre, Newfoundland.
Stacey, George B. . .	Eastern Telegraph Company, Bombay.
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Stevens, Henry . .	Port Darwin, South Australia.
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Stevenson, George . .	Eastern Telegraph Company, Alexandria, Egypt.
Stewart, D. . . .	Postal Telegraphs, Glasgow.
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Stone, William H. . .	Department of Public Works (Telegraphs), Tokio, Japan.
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Tansley William . .	Postal Telegraphs, Portarlington.
Tapp, George R. . .	18, Queen Margaret's Grove, Mildmay Park, N.
Taylor, Frank A. . .	Eastern Telegraph Company.
Taylor, Henry . . .	Western and Brazilian Telegraph Company, Rio de Janeiro, Brazil.

TAYLOR, WILLIAM GRIGOR . .	Eastern Extension Telegraph Company, Sydney.
TEALE, WALTER	
THWAITES, JAMES . . .	25, St. John's Street Road, Clerkenwell, E.C.
THEILER, RICHARD . . .	86, Canonbury Road, N.
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TUBB, ALBERT	Postal Telegraphs, Southampton.
TUCK, W.	Eastern Telegraph Company, Suez, Egypt.
TUFFIELD, T. S.	Agencia de l'Empresa, Telegrafo Submarine, Arica, Peru.
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TYLER, W. J.	106, Cannon Street, E.C.
UPTON, HENRY	53, Grey Street, Newcastle-on-Tyne.
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VESEY, ARTHUR H. . . .	Widford, Hertfordshire.
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WEATHERALL, T. E. . . .	Telegraph Construction and Maintenance Company, Greenwich.
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WESTON, ROBERT W. GIBBS . .	Bervie House, Thurloe Place, Lower Norwood.
WHITE, F. E.	Anglo-American Telegraph Company, St. Pierre, Newfoundland.
WHITMORE, MORTIMER, Lieut., R.E. .	Postal Telegraphs, Bristol.
WHITAKER, J. E. . . .	Inspector Lancashire and Yorkshire Railway, Halifax.
WHITTINGBACK, C. H., Sergt., R.E. .	Indo-European Government, Telegraphs, Sivand, Persia.
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WILKES, HENRY F. . . .	2, Castle Chambers, Belfast.
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WILLIAMS, A. G. . . .	Anglo-American Telegraph Company, Heart's Content, Newfoundland.
WILLIAMS, J.	Indo-European Government Telegraphs, Persia.

WILLMOT, JOSEPH . . .	Postal Telegraphs, General Post Office, E.C.
WILMOT, T. J. . . .	Direct United States Cable Company, Rye Beach, U.S.
WILSON, C. H. . . .	Eastern Extension Telegraph Company, Hong Kong.
WINDELER, HENRY . . .	Anglo-American Telegraph Company, Valentia, Ireland.
WINTER, C. E. . . .	33, Grosvenor, Bath.
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WOOD, Major ALEXANDER . .	Abbey Wood, Kent.
WOODCOCK, W. . . .	Anglo-American Telegraph Company, Heart's Content, Newfoundland.
WOODS, JAMES W. . . .	Engineering Department Postal Telegraphs, Oxford.
WRIGHT, JAMES . . .	Messrs. Siemens Bros., 12, Queen Anne's Gate, S.W.
WRIGHT, THOMAS . . .	The Eastern Telegraph Company, Malta.
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Total Number of Associates . . . 475

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ST. GEORGE, A. F.	Montagu Villa, Redhill, Surrey.

Total number of Students 11

TOTAL NUMBER OF MEMBERS.

Honorary Members	4
Foreign Members	152
Members	339
Associates	475
Students	11
Total	981

JOURNAL

OF THE

SOCIETY OF

Telegraph Engineers and of Electricians.

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The Ninety-sixth Ordinary General Meeting of the Society was held on Thursday evening, February 24th, 1881, at the Institute of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. FOSTER, F.R.S. (President), in the Chair.

The minutes of the last meeting were read and confirmed, and the names of transferees and new candidates announced.

The SECRETARY then read the following communication:—

"LONDON, *February* 18*th*, 1881.

"EARTH CURRENTS.

"On the 1st of April, 1878, I published in the *Telegraphic Journal* a series of earth-current observations on the Scotch-Norwegian cable. The results I then arrived at, through observations carried out every hour for about two months, were as follows:—

"1. That the current changes direction about every six hours, *i.e.*, four times in the twenty-four hours.

"2. That the normal maximum strength of the current scarcely ever exceeds 3 to 4 Leclanché's cells, *i.e.*, an E.M.F. of about 5 volts.

"3. That the current after each change of direction increases

gradually from a minimum to a certain maximum strength, and then again decreases, until it finally assumes an opposite direction.

"Not being able, through further observations, to obtain any other results, the work was discontinued for some time, and was only resumed last winter, when it was noticed and pointed out to me that the change in the direction of the current *coincided with that of the tidal wave.*

"CHR. DRESING, Member."

Mr. DRESING, on being asked if he desired to supplement his paper with any remarks, said: I have very few remarks to make beyond those which the Secretary has been kind enough to read. It may be to the interest of the Society to know that the observations related were carried out by means of an ordinary galvanoscope, but a rather sensitive one, of a resistance of about 1,200 units. It was clearly proved, after about a fortnight's observations, that the current changed in direction every six hours; but it did not occur to me at the time that that result was in any way connected with the moon. Lately, however, one of my friends, who is stationed at the Scotch end of the cable referred to in my note, found out that every time the tide along the coast changed, the direction of the galvanometer deflection was changed also—that for every three hours the deflection gradually increased until it attained a certain maximum, when it gradually decreased for another three hours, when it reached zero and resumed an opposite direction.

Mr. W. ELLIS (Greenwich Observatory): I was not present when Mr. Adams' paper was read, nor do I know what discussion took place upon it, but I have thought that it might be interesting to the members of the Society if I were to describe what occurred at Greenwich on the occasion of the magnetic storm of the 31st of January last, as illustrating the connection between earth-current and magnetic disturbance.

With the permission of the Astronomer-Royal, I have brought with me this evening the photographic registers for that day. I am sorry to say that the registers are on so small a scale that they cannot be seen at a distance; but I will hand them to the Council for present examination, and shall be happy to offer explanations

of the indications to any members who may desire to inspect them at the close of the meeting.

The photographs exhibit the variations of magnetic declination, of magnetic horizontal force, and of magnetic vertical force. The earth currents are those registered in two lines, one extending from the Observatory to Angerstein Wharf, situated on the river bank, near Charlton; the other extending from the Observatory to the south end of the Blackheath tunnel.

On January 31st, 1881, disturbance became marked between 3 and 4 p.m., as appears by all the magnetic and by both earth-current registers. Great activity continued to be shown until between 8 and 9 p.m., the disturbances, both magnetic and earth-current, all fading away together. Then, after a time of quiet, between midnight and 1 a.m. on the following morning, there was a sharp bend shown on all the magnetic registers, with corresponding sharp bends in both of the earth-current registers. No better example than these sheets afford can be given of the circumstance that magnetic and earth-current disturbances are practically simultaneous: they begin together and end together, and it is this point that I wish to impress strongly to-night upon the attention of members. It is what always occurs: unusual magnetic disturbance is always accompanied by active earth currents. There appears to be no exception to this rule, and examination of any number of sheets would only corroborate the evidence given by these. The same result is always seen. As contrasting with the records for January 31st, I have brought those for the previous January 13th: it will be seen that the registers are all alike quiet, the earth-current traces being nearly straight lines. Thus earth-current activity and magnetic activity go entirely together. The appearance of aurora in our latitudes is also always accompanied by magnetic disturbance. But we may go a step further. If I were to ask any person, not conversant with magnetism or astronomy, to pick out from our photographic records for the last thirty years the periods in which the curves are boldest, and in which there is most activity shown, he would, with little hesitation, select the periods in which the sun is known by astronomical observation to have manifested in the greatest degree that energy

which is made visible to us by the increase in the number and magnitude of solar spots. These four forms of energy appear to be thus all related. The relation in time between magnetic and earth-current disturbance is complete, even in details, but the relation with solar change seems, so far as has been ascertained, to be more general. It is on record that sudden outbursts on the solar surface have been accompanied by immediate magnetic disturbance, but whether every change that occurs in the sun in disturbed times has its counterpart in magnetic disturbance on the earth, or, indeed, whether it should be expected to find its exact counterpart, seems a matter for further inquiry, although the general relation between disturbed periods and quiet periods is most marked.

I should like to corroborate what Mr. Whipple of the Kew Observatory has quite recently made known in *Nature*, which is, that the electrical condition of the atmosphere near to the earth's surface, as indicated by Thomson's self-recording electrometer, appears to be in no way unusually affected at the times of magnetic storms.

Perhaps I might add a few words in regard to magnetic inequalities of regular period. This information I take entirely from the *Greenwich Observations*.

At the Royal Observatory, the magnetic variations are measured by three magnets: one, the declination or ordinary variation magnet, placed in the magnetic meridian; one, constrained by torsion to take a position transverse to the meridian, for measure of variations of horizontal force; and one, supported on knife-edges, for measure of variations of vertical force.

First, as regards solar inequalities. The diurnal inequality exhibits principally one maximum and one minimum. The western declination maximum occurs, on the average, about 1 or 2 p.m., and the minimum about 8 a.m., the difference between the extreme positions being about 9 minutes of arc. In different years and in different seasons of the year, these particulars somewhat vary. The horizontal force is, on the average, greatest at night and least rather before noon, the difference between its greatest and least values being about $\frac{1}{500}$ part of the whole hori-

zontal force. The vertical force appears to be greatest by day and least by night. As regards lunar inequalities, there appears to be no distinct inequality dependent on the age of the moon; but there is a sensible inequality depending on the position of the moon with regard to the meridian, its character, unlike the solar inequality, being mainly semi-diurnal, that is to say, it goes through its changes twice in the lunar day. Maxima occur both with regard to declination and horizontal force at about 1 or 2 a.m. and p.m., roughly speaking, and minima at about 7 or 8 a.m. and p.m., the variations being much smaller in magnitude than the solar variations. The Astronomer-Royal pointed out that these lunar variations might be represented by one force which is alternately positive and negative, and which goes through its changes twice in the lunar day, the direction of the force being north-west and south-east (magnetic) nearly. These results are to be found in the *Greenwich Observations* for 1859 and 1867. In a paper published in the *Philosophical Transactions* for 1870, the Astronomer-Royal pointed out the existence of a diurnal inequality as regards earth currents, but the results were not discussed with respect to the position of the moon; and in another paper to be found in the *Philosophical Transactions* for 1868, he made a comparison between magnetic storms and earth-current disturbances. I allude to these earth-current papers in order that it may be known that such papers exist, and that members can refer to them. I beg to thank the meeting for the attention given to my few remarks, and repeat that I shall be extremely happy to describe the photographs to any of the members at the close of the meeting.

Mr. S. M. BANKER: I take it that a theory, to be of any service, should be capable of application to existing earth-current data, and not to one particular series of observations. If we look at Professor Barlow's results in 1848, when he took readings on two wires at every five minutes for fourteen days and nights, at Derby, and published in the *Philosophical Transactions* for 1849, we find that the maxima and minima occurred about the same time in each twenty-four hours. In the *Philosophical Transactions* will

also be found papers on the same subject by Mr. C. V. Walker,* Professor Balfour Stewart,† and by the Astronomer-Royal.‡ In the *Transactions of the Royal Irish Academy* are papers by Professor Lloyd,§ and many other papers in other scientific publications‖ on the same subject, not forgetting a valuable one by Mr. J. Graves, of Valentia, in the Journal of this Society.¶ They all point to one conclusion, that there are certain fixed hours in the twenty-four when the current is found to be at a maximum, and certain fixed hours when it is at a minimum, and my own observations have led me to the same conclusion. Perhaps the most conclusive papers on the subject are the two by the Astronomer-Royal in the *Philosophical Transactions* for 1868 and 1870, where he has given the daily curves, produced by photography, of the declination magnet and the magnet actuated by the earth currents flowing through a wire which extended from Greenwich to Croydon. The photograms in each case for the same periods are almost identical, both in the greater and lesser curves; a greater activity, however, is displayed by the earth-current curves where slight variations take place which the declination magnet seems incapable of suddenly responding to. I need scarcely say anything in reference to the declination magnet more than state "that it is generally recognised that the regular solar-diurnal variation, when freed from the influence of disturbances, has the same or nearly the same turning hours in all parts of the northern hemisphere which are beyond the magnetic equator, viz., an extreme easterly elongation about 8 a.m., and an extreme westerly elongation about 2 p.m."**

A point which requires further elucidation in Plate 3, is where these earth currents are stated to follow the moon by three or four hours. Now, if the earth has a free charge, would it not

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* 1861 and 1862.

† 1861 and 1862.

‡ 1868 and 1870.

§ 1861 and 1862.

‖ *Trans. Royal Society Edin.*, 1863—Professor Balfour Stewart. *American Science and Art Journal*, 3rd Series, 1859, 1860, 1861—Professor E. Loomis. "A Handbook of Practical Telegraphy," by R. S. Culley, 7th edition.

¶ Vol. 2, 1873.

** General Sabine, "St. Helena Observations," Vol. 2.

follow directly under the moon? supposing the moon to be the cause of these currents. We have evidence to show that electric force travels as fast as light itself;* therefore, if the earth has a free charge, it would be under the moon by inductive action. I would also ask how the three other currents are to be set in motion, and what force keeps them in their relative positions?

We have also presented to us lunar-diurnal curves, on Plate 6 of several places of observation; but there was one thing which Mr. Adams omitted to point out in reference to them, and that is, where is the moon when the greatest westerly or easterly lunar deflection takes place? It is this:—When the moon passes the meridian of Greenwich, one hour afterwards the greatest westerly lunar deflection takes place,† whereas by the theory it should be three or four hours. When the moon passes the meridian of Dublin it coincides with the greatest westerly deflection,‡ and, if we go on to the meridian of Toronto, we find when it reaches there the greatest easterly lunar deflection.§ Again, when the moon passes the meridian of Hobarton and St. Helena, the hours of maxima and minima differ from each other and from the others, so much so that Professor Lloyd‖ has arrived at the conclusion, with respect to the so-called lunar deflections, that lunar action is its remote and not its immediate cause. General¶ Sabine also says, from Toronto and Falkland Islands being both easterly or westerly deflected together, while Pekin and Hobarton are both deflected together in the same direction but opposite to the two former, that the diurnal-lunar variations appear to divide the earth into east and west hemispheres, and not north and south. This, I think, is against the lunar theory of earth currents.

I believe I can explain how these currents are formed by a sketch.

Let the circle represent the earth, N S the magnetic poles of

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* *Phil. Trans.*, 1861—Professor Balfour Stewart. *Royal Astronomical Society's Proceedings*, 1859.

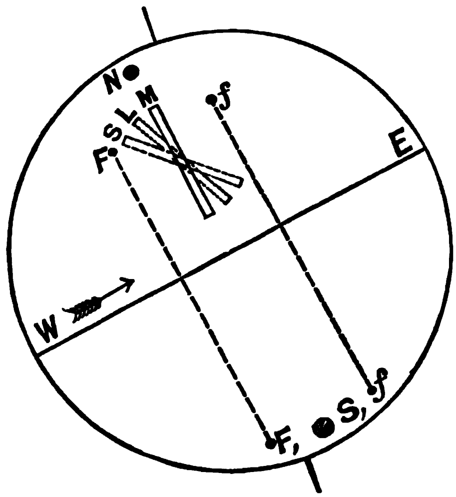
† "Magnetical and Meteorological Observations, Greenwich," 1878.

‡ "A Treatise on Magnetism," by H. Lloyd, D.D., D.C.L. 1874.

§ Ibid. ‖ Ibid.

¶ "St. Helena Observations," Vol. 2.

the earth, W E the magnetic equator, M represent a declination magnet, L the same magnet when deflected by the greatest lunar-



diurnal force, and S the same magnet when it is deflected by the greatest solar-diurnal force. Now, the greatest lunar-diurnal force is about 10 seconds of arc as a maximum, and this, according to the theory, is caused by the earth currents—that is, one produces the other; but the greatest solar-deflection is, as a maximum, about 10 minutes of arc, or 60 times greater than the lunar deflection. This deflection has not been taken into account.

To move M to S, it is necessary that the magnetic influence should be increased on one side or decreased on the other to cause the movement of the needle in the direction indicated: this can only be accomplished by some different arrangement in the lines of force. Now, the earth being a permanent magnet so far as we are at present concerned, it has innumerable lines of force proceeding from its surface in all directions, to re-enter it again, in all probability, after travelling some thousands of miles through and above our atmosphere. In sketch, let F *f* represent the plan or points where two such lines or tubes of force leave the earth, one on each side, so as to balance the magnet. If the line of force, *f*, which acts on the right-hand side is by any means twisted out of its course, or weakened whilst the left-hand force, F, remains in its

normal state, the equilibrium would be destroyed, and the magnet would be drawn to the left; but there is something else which must occur when the right-hand force, *f*, is being weakened, and it is this: *a current is formed in any conductor which may be passing through that line of force at the time.* This is one of the laws of electro-magnetism, and I do not think it has been fully appreciated when earth currents have been investigated. To proceed, it may be asked how this line of force is to be weakened or twisted. Professor Faraday proved that oxygen, which forms a portion of our atmosphere, is magnetic, and any magnetic substance when heated loses a portion of its magnetic power, so that our lines of force emanating from the earth would magnetise the atoms of ogygen and arrange them in order, forming a magnetic circuit; but when the sun shines on these atoms of oxygen, it would weaken the line of force, *f*, to the east first, upset the equilibrium of M, and produce the deflection of the magnet, and as a natural consequence the current: therefore what is true for one line or tube of force is true for the innumerable lines of force proceeding from the earth, and the conductors which these lines of force cut is the surface of our earth and all conducting material whatever on it. The heat from the sun, which affects these lines of force, is communicated slowly downward from atom to atom, so as to produce a succession of infinitesimal changes in the conducting medium of the lines of force and in the current so produced; but the whole summed together is what we get shown by the declination magnet and the currents on our sensitive galvanometers. These are the currents, I believe, which we have under discussion, are based on known laws, and must form an integral part of their consideration.

It is readily conceivable, from what I have just stated, that these currents are affected by the nature of the countries where they are recorded, as the difference in radiation and absorption of heat is vastly different in rocky or wooded lands; also whether there are any permanent or periodical winds; whether the weather is hot, wet, or temperate at intervals; whether it is an inland station or one situated on the sea coast—all tend to make these currents different in different localities. Say, for instance, the lines of force are being gradually heated, producing a steady current, when suddenly a cold cloud

rushes into these lines of force, and at once alters their magnetic power, and as a consequence produces an induced current in the opposite direction to that previously in existence. At one time I could make a fair guess as to the stability of the earth currents on the wires, by paying attention to the weather. Therefore, the variations which we get in these currents are caused by differences in temperature or the thermo-differential state of the atmosphere, and, if studied sufficiently, a law may be derived to indicate the quantity of heat lost or gained, and it follows that we should get greater currents in summer than in winter, travelling side by side with the thermo-curve. The direction of these earth currents are determined as follows:—To produce the magnetic poles of our globe would require a positive current flowing from west to east nearly, and any conductor made to cut the lines of force so produced would have a momentary current flowing from east to west; when the conductor is withdrawn, another momentary current is produced, flowing west to east. It matters not whether it is the inductor or the conductor that is moved, the effect is the same. Hence it follows that if the lines of force are weakened or strengthened, we get induced currents in proportion to that weakening or strengthening, and their direction is the same as if the whole magnetism had been quenched or just produced. These directions agree with most of the earth-current data.

I believe the whole of the diurnal movements can be explained without difficulty, principally by thermal causes.

It is not generally known that these earth currents can and do interfere with the working of delicate apparatus in the postal system. There is a circuit working on the fast speed automatic system between London and Cork which runs in a most favourable direction for earth currents. I have known it for a fortnight at a stretch, in summer time, to commence work with a certain speed at 10 a.m. and to keep it up until about 11.30 a.m., when it had to be reduced until nearly 2 p.m., after which time the speed would again go up. Of late years a translator has been placed on the circuit, which prevents the speed going down. I do not think the lunar or tide theory will account for this.*

* 63 miles of cable in the circuit across the St. George's Channel.

I have endeavoured to point out a few objections to the lunar theory, and a reason for a solar theory; however, if Mr. Adams' earth-current curve is free from polarisation and other disturbances, it is passing strange that for upwards of sixteen years the earth currents have been photographed daily at the Royal Observatory, Greenwich, and yet the greatest deflection has not been noticed to follow the moon. 1870* was the last time, so far as I can trace, that any public announcement was made on this subject by the Observatory. If I am wrong on any point I shall be most happy to be corrected.

Mr. WILLOUGHBY SMITH: At our last meeting Mr. Wollaston stated that Professor Faraday had informed him that if a wire were suspended between Shakespeare's Cliff, Dover, and Cape Grisnez, on the opposite coast, the motion of the tide beneath would induce currents of electricity in the wire. I think Mr. Wollaston must have misunderstood the Professor. Were it possible to so suspend a wire, no doubt the motion of the same would induce magnetic currents; but I do not believe it would be in any way affected by the motion of the water beneath. I also think that Mr. Wollaston's data must be incorrect as regards the gutta percha covered wire submerged between the same points in 1850, as no experiments for earth currents were made on that line. The multiple cable laid the following year contained four wires, each separately covered with gutta percha. Two of the wires were employed for ordinary work, and the other two Mr. Wollaston says he experimented with for earth currents. Now, wires so placed are not favourable for such experiments, as the induced currents from the two wires in which strong battery currents were passing would vitiate the results.

To enable me to speak with more confidence as to any effect of the tide on a submerged wire, I have had, since our last meeting, two wires, 500 yards each in length, laid across the Thames, at right angles to the run of the tide. One of the lengths consisted of a copper wire, covered with gutta percha only; the other length had an additional covering of hemp and iron wires. The distant end of each length was sealed, or insulated, and the near end of

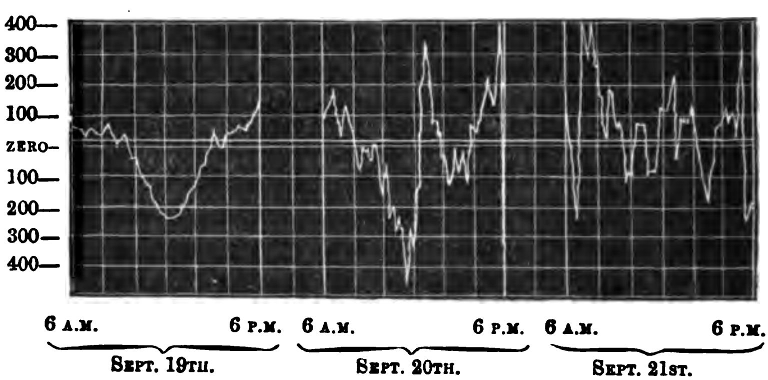
* *Philosophica Transactions.*

each connected to earth through a very sensitive astatic mirror-reflecting galvanometer. The galvanometers were constantly watched for twelve hours, and during that time not the slightest deflection was observed, although half a division on the scale would only have been equal to the $\frac{1}{87000}$ part of a volt. I, therefore, am of opinion that, whether part of the wire Mr. Adams employed for his experiment passed under or over the Severn, it was in no way affected by the ebb or flow of the tide.

As to the curves Mr. Saunders has handed in of the readings taken on the 335 miles of Atlantic cable laid in 1857, I do not think much reliance should be placed on them, for the simple reason that the length of cable was in a very imperfect electrical condition throughout. But the other curves which he has also given are valuable, as the results they record have been obtained under most favourable circumstances. There is no person who has such opportunities for obtaining data as to the effect of earth currents on cables as Mr. Saunders, and I hope he will one day give the Society the benefit of his experience, in the form of a paper.

In attempting to lay a cable in 1865 between Ireland and Newfoundland, the cable parted after 1,200 miles had been successfully laid, and the end was lost in 2,000 fathoms. No. 1 diagram

No. 1.

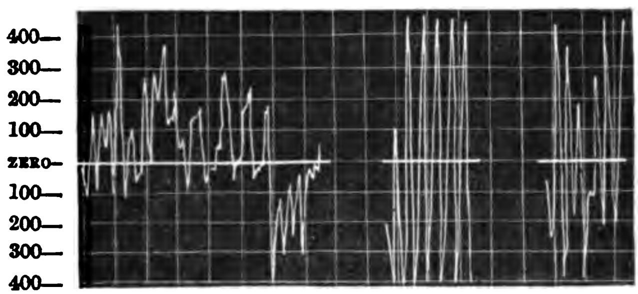


shows the readings taken at Valentia on a mirror galvanometer connected to the shore end of that cable, the other terminal of the galvanometer being attached to earth by means of the iron wires

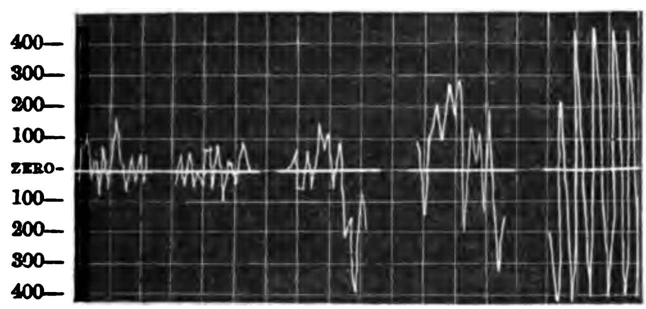
of the cable. The readings were observed for three consecutive days, from 6 a.m. to 6 p.m. each day. Although on paper the results look rather formidable, the maximum deflection does not denote a higher tension than the $\frac{1}{8}$ of a volt.

Last September 288 miles of cable were laid in the North Sea. The distant end was insulated and the shore end connected as in the experiment with the Atlantic cable. The results obtained

A No. 2. B C



D E F G H



are shown in diagram No. 2, and the following are the conditions under which these observations were noticed:—

A.	September	14th,	1880,	5 p.m.	Wind	E.,	sea moderate to smooth.	
B.	„	16th	„	8 p.m.	„	S.W.,	sea very rough.	
C.	„	17th	„	„	„	„	light, sea moderate,	weather fine.
D.	„	18th	„	„	„	„	„ sea very calm,	„
E.	„	19th	„	„	„	„	„ sea smooth,	„
F.	„	20th	„	„	„	N.W.,	„ „	„
G.	„	21st	„	„	„	W.,	„ „	„
H.	„	22nd	„	„	„	S.E.,	„ sea moderate,	„

I have no theory to offer on the subject; all I know is that the more data I obtain, and the more I examine such data, the more confused I become.

Sir CHARLES BRIGHT: Do I understand that in these observations the copper was exposed to the sea?

Mr. WILLOUGHBY SMITH: Yes; at the lost end only.

Sir CHARLES BRIGHT: That was a fault—a bad fault?

Mr. WILLOUGHBY SMITH: Equivalent to a fault.

Sir CHARLES BRIGHT: It was a broken cable?

Mr. WILLOUGHBY SMITH: Yes; 1,200 miles of perfect cable, with the conductor at the distant end exposed to the water, and generating a current between the iron and copper of the cable, which has to be taken into account. Under these conditions there would be a steady deflection in one direction only, due to the difference in potential of the two earths, viz., copper and iron immersed in salt water.

Mr. C. F. VARLEY: What are the vertical figures?

Mr. WILLOUGHBY SMITH: Deflections on the galvanometer scale.

Sir CHARLES BRIGHT: What was the condition?

Mr. WILLOUGHBY SMITH: In this case, 288 miles of perfect cable with distant end sealed or insulated.

Sir CHARLES BRIGHT: Where was the other end?

Mr. WILLOUGHBY SMITH: To earth on shore through a galvanometer.

Sir CHARLES BRIGHT: I thought it was taken from the ship?

Mr. WILLOUGHBY SMITH: No; on shore. Experience leads me to think that the phenomena called earth currents are at times quite local in their action. On the completion of the laying of the Atlantic Cable in 1866, its electrical condition was being tested at midnight, and on that occasion at Valentia the weather was fine, but at Newfoundland very stormy, raining and blowing very hard. At Valentia there was no electrical disturbance to interfere with the tests, but at Newfoundland it was impossible to make the necessary tests, there being a constant flow from the cable equal in tension to over 100 volts, and whether the distant end was free or to earth the results were in no way affected. I am of an

opinion that the results obtained from one line only will never give any clue to the laws that these currents follow. If it were possible to arrange for all the submarine cables and all land lines to be used at the same hour of each day, to measure the amount and direction of earth currents on each, then perhaps we might obtain reliable data to work upon. But the results from one isolated line will not, I think, enable us to arrive at the goal we are all anxious to reach.

The PRESIDENT: Before calling upon any one else to continue the discussion, I should like to ask Mr. Willoughby Smith for a little more information on one point which seems of importance—that is, as to the copper wire he referred to laid across the Thames. I understand that one end of each wire was insulated?

Mr. WILLOUGHBY SMITH: Yes.

Professor W. E. AYRTON: One end of each of the two wires?

Mr. WILLOUGHBY SMITH: Yes.

Professor W. E. AYRTON: Neither wire put to earth at both ends?

Mr. WILLOUGHBY SMITH: No; the far ends insulated in both cases.

Mr. EDWARD GRAVES: Perhaps I may be allowed to correct a trifling inaccuracy in a matter of fact into which the meeting fell during the last discussion. I was not present, but I am informed that when Mr. Wollaston asked Mr. Adams whether a portion of the circuit from London to Cardiff, on which some of his observations were taken, passed through a submarine cable, Mr. Adams (under the impression that there was a short submarine cable crossing the Severn at Gloucester) said that such was the case, and Mr. Wollaston drew certain deductions from the alleged fact. I have simply to say that there is no submarine cable, not even a yard of submarine wire, on any circuit between London and Cardiff.

Mr. C. WOLLASTON: But exactly the same effect would be observed by the water running under the wires. Mr. W. Smith, I think, has told us just now that the more he investigates the less he seems to know. Now I cannot conceive that a gentleman with the immense experience of Mr. Smith tried such an experiment as he states he has tried across the Thames since your last

meeting, and soberly laid it before you as an experiment in any way connected with that to which I alluded, from one side of the Channel to the other, at your last meeting. Mr. Smith has just mentioned that the end of the wire across the Thames was sealed. How, I ask you, did he ever expect a current to be shown similar to the current shown by the action of the tide setting up and down the Channel between Dover and Calais, across a submarine line connected with the earth at both ends? The circumstances seem to me to be totally different. I stated at your last meeting that it was only an hour or so before your meeting that I was aware that a discussion was likely to take place on the question. I came down and very briefly mentioned the experiments which I tried in 1851. I did not mention any experiment whatever having been tried upon the cable which Mr. Smith alluded to as the first cable of 1850. It is very well known that that was an experimental cable, and it was severed soon after it was put down. As regards the 1851 cable, I did not state that I used two wires for the experiments, but that two wires were used for work, and that upon the other wires I tried experiments. But I also stated that I connected a galvanometer in circuit at Dover with the cable wire to one terminal, and the other terminal connected to the earth: the far end of the cable at Calais being to earth. I also said the subsequent experiments confirmed the previous experiments that it was owing to the setting of the water across the cable that the currents obtained between Calais and Dover were to be attributed. The wire in these later experiments was laid from the telegraph works at East Greenwich, and the same galvanometer was employed as in the former case. I did not seal a wire, put it into the Thames, and expect to get any result upon the galvanometer. I connected the far end of the wire to the earth, and at the home end a galvanometer placed in circuit and then joined to the earth, expecting to get similar results to those previously obtained between Dover and Calais, and I did get them. The water set up the Thames, and the needle went in one direction: the water set down the Thames, and the needle went in the other direction.

I am very sorry that the recent bad weather has prevented my repeating similar experiments, as, if I had been enabled to do so,

I think that the results would perhaps have convinced even Mr. Willoughby Smith on the point. I think the action is due to the setting of the water, and the paper which the Secretary read at the opening of the discussion this evening confirmed that idea. It is not the changing of the moon, but the setting of the tide across the wires. I may mention that when in South Africa (where I have been occupied in Cape telegraphs for many years) I had the opportunity of observing exactly similar effects caused by the setting of the river Gamboos upon a galvanometer needle when the wire was of long suspension, and very nearly (sometimes quite) in contact with the surface of the water. I watched again and again very carefully, and found that perhaps at 2 o'clock in the day the needle was over in one direction; at 2.40 the next day a corresponding movement; 40 minutes later a corresponding movement the next day, and so on, which perfectly confirmed me. But as a little practice is worth an immense deal of theory, there is a gentleman present connected with the Submarine Telegraph Company, and I am sure, if he connected up one of their wires in the way I did, he would obtain results confirming mine. What I mentioned on the last occasion I restate now. I am not in error as to what Professor Faraday said to me, and I am quite confident myself, and think many who are now perhaps sceptical will come to the same conclusion, that a very great number of these so-called earth currents are caused by the flow of water very, very much less in magnitude than the bulk of water generally supposed capable of producing much influence when passing under suspended wires.

Mr. C. E. SPAGNOLETTI: I have collected a little information touching upon Mr. Adams' paper with regard to the earth currents on the night of 31st January—not on the question of their cause, but effects. I have a report from my Inspector of the Worcester Division, who says:—

"In watching the aurora on the night in question, I noticed a singular thing, one that I had never seen before in connection with the aurora—the arc of the light was perfect, the oblique rays in regular formation, shooting straight up from the north, and then darting away to the east and west, but on the west side two

oval rays of luminous light shot out towards a very bright star that is now seen of an evening—(Mr. C. F. Varley: The planet Venus)—travelling westward, and from those two ovals two rays of light shot up, and it appeared to me that the star had some attraction and drew off some portion of the aurora. It was a beautiful sight, the sky here being perfectly clear, and I wish I was draughtsman enough to give you a good sketch, as I feel sure you would be interested in the phenomenon.

"J. MILLER."

Mr. Spagnoletti remarked that the two ovals of light were probably due to reflection similar to that which occurs in the double rainbow.

"EARTH CURRENTS.

"*January* 31*st*, 1881.

"Paddington.—At 6.40 p.m. the Windsor and Maidenhead local circuits, Birmingham, Swindon, Exeter, Oxford, Hereford, and Wolverhampton circuits, held over to right. At 6.55 Birmingham and Wolverhampton changed to left, other circuits remaining with needle deflected to right. At 8.15 current disappeared. A short circuit to Hammersmith and between Paddington and the City was not affected.

"Reading.—Strong deflection to right on all circuits at 6.30, except P.O.; impossible to work. At 6.45 some circuits changed to left; at 8.0 p.m. deflection had disappeared. Horizon very clear; aurora, from 6.30 till 6.50, showing brightly; no wind.

"Didcot.—The needles of some circuits affected about 4.0 p.m., all at 7.0 deflecting to right, and varying, as if in contact, then appearing clear. The Oxford circuit, which was deflected to left, I put to earth on the up side of Didcot. Needle went to the right; when earth was removed needle went to the left, acting in the same way when I put on down side. All right at 8.0 p.m.

"Oxford.—Needles held over about 5.15; over at about 8.

"Birmingham.—4.10, Paddington and Birmingham and Birmingham and Birkenhead circuits' needles deflected; at 5.0 all circuits affected; at 8.0 p.m. all clear.

"Shrewsbury reports all circuits affected at 3.45, clear at 10 p.m.; effects variable; P.O. circuit not affected. Never saw instruments so much affected before by earth currents.

"Chippenham noticed variable deflections.

"Worcester.—4.0, deflections observed; strongest at 6.40 to 7.0, when all needles firmly held over; the Worcester and Salop circuit the strongest. Current variable, sometimes slowly and sometimes quickly changing from right to left. Currents appeared strongest on lines running N. to S.; ceased at 10.0 p.m.

"Hartlebury.—Very strong deflections; bells occasionally rung.

"Bristol.—4.30, deflections right and left; very strong from 6.0 to 7.0; disappeared at 8.30.

"Neath.—Earth currents observed from 11.0 a.m. to midnight: needles changed from side to side; wind S.S.E.; lines running from E. to W. Deflection strongest on Swansea and Merthyr circuit (this runs S. to N.); needles carried from right to left till 6.37, when Swansea and Merthyr remained over to left till 7.5, when it changed to right; then all circuits appeared clear until 7.12, when lines running E. to W. again deflected to left, and N. and S. to right. 7.27, Gloucester and Swansea (E. and W.) circuit deflected slightly to right; Swansea and Merthyr (N. and S.) circuit to right, and continued to vary right and left, and *vice versa*. 7.40, all clear. A short circuit about half-a-mile not affected at all.

"Swansea.—3.30, E. and W. circuits affected, varying right to left. All circuits at 5.0 p.m. affected except P.O.; unable to work at times; disappeared about 1.5 a.m., February 1st.

"Carmarthen Junction.—Down line circuits, needles to left; up line circuits, needles to right; through circuits, needles to right."

It will be noticed by these observations that all the short circuits were not affected, showing that the potential for short distances did not vary; and I think it would be very interesting if anybody could tell us at what distances the variations do or have been observed to occur.

With regard to the effects of water flowing by or under an

insulated wire, I had some experiments tried in the River Thames at Moulsford, and the following is my Inspector's report :—

"18*th February*, 1881.

"I have been to Moulsford to-day and made the undermentioned tests. I obtained 40 degrees deflection to the right and 36

FIG. 1.

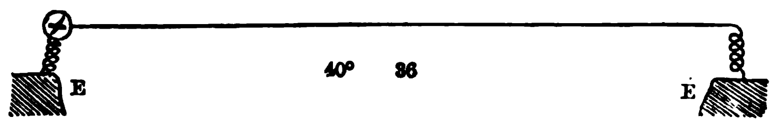


FIG. 2.

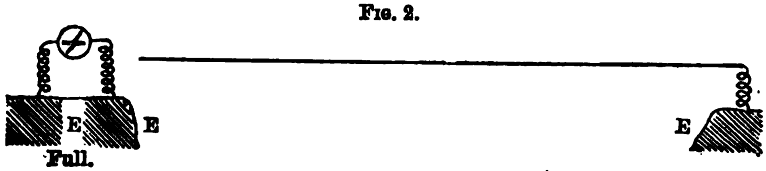


FIG. 3.

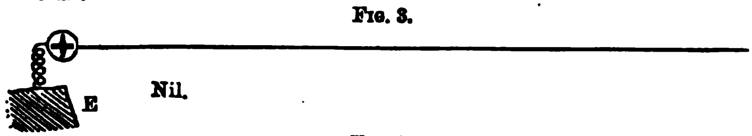


FIG. 4.

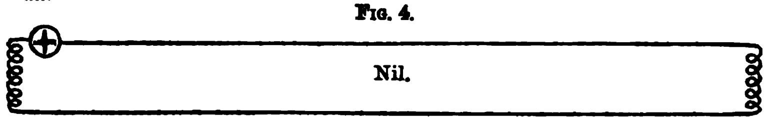


FIG. 5.

ABOVE WATER.

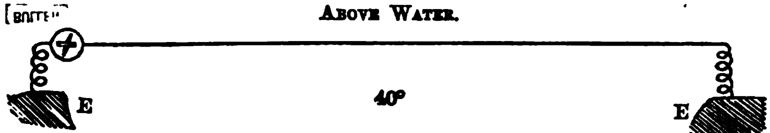
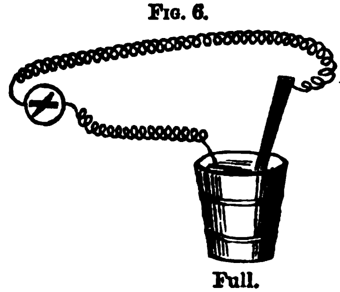


FIG. 6.



to the left on astatic detecter, with a No. 7 G.P. wire (Fig. 1) immersed in the Thames from one side of the river to the other. I

afterwards removed the line wire from galvanometer and substituted an earth wire in its stead (Fig. 2). The result was the same as the previous test, thus plainly showing that a local battery was set up by the two dissimilar metals, viz., the copper wire and the galvanised iron earth rod, aided by the moisture; I disconnected the line wire on the opposite side of the river (Fig. 3), but did not get the slightest deflection. I then laid down another line wire (Fig. 4), and so avoided any earth connection, but the needle still stood to zero; and lastly I raised the line wire (Fig. 5) out of the water, using the same earths, and received the same deflections as Fig. 1. I used two galvanised iron rods driven into the bank on either side of the river for earth connections, which passed through some water before penetrating the soil; but when I tested on a previous day I used two wrought-iron bars and observed the same deflections then as I did to-day. The stream travelled near about the same rate as it did on the previous day, and the weather was dull and damp, with little or no wind. On my return to Reading, I placed a galvanised rod into a wooden bucket filled with clean water only (Fig. 6), connected a copper wire from this rod on to astatic, and another thence into the water. The result was 75 degrees deflection, and the more copper there was immersed the higher seemed to be the movement of the needle; I then tried a wrought-iron rod in the same manner, but only got 50 degrees with that."

Not satisfied with this, I sent the same officer to Brentford, and I must say that the experiments there were not so satisfactory, because he only tried the experiment when the tide was running one way. He reported:—

"*February* 24*th*, 1881.

"I went to Brentford this morning, and laid a No. 7 G.P. wire across the Thames at Kew Bridge, immersed from one side of the river to the other as desired, using a copper plate buried on either side in the bank for earth, but the needle of the astatic was not deflected in the least, neither was it when the line wire was lifted out of the water. I afterwards placed the wire on land and tested with an earth at each end, but the result was the same, the needle still stood to zero.

"I may add the tide had been going out about an hour when

I arrived, and was running about two miles per hour, so that I was only able to make the test one way of the stream, instead of two as proposed.

"The G.P. wire was perfect in every respect."

It therefore appears that, when galvanised and iron rods were used (Figures 1 and 5), a deflection was observed of 40°, but when copper plates were used none were observed. Figure 2—by putting copper wire to earth and galvanised iron rod, a battery was formed.

Figure 3 would not show a deflection, not being a complete circuit.

Figure 4, having the two wires, if any action took place, the same force acting on them would set up a current in each wire, and equilibrium would be established, hence no deflection.

Figure 6 would be a battery cell again.

These remarks may, I trust, be interesting to some of the members.

Mr. G. K. Winter: Some of you will remember that about eight years ago I read a paper before this Society on the subject of earth currents, describing my observations on the subject, and giving, in general terms, the results obtained.

There is an important point connected with the observation of earth currents to which, I think, too little attention has been paid. By observing the currents in one wire only, we are unable to distinguish those changes which are due to variations in the absolute strength of the current passing through the earth, from those which are due to variations in the direction of the earth stream relatively to the line joining the earth plates, of the wire under observation. In order to distinguish between the effects of these two causes, it is necessary that simultaneous observations should be made on two wires running in different directions.

At Arconum (about 40 miles inland from Madras), where most of my observations were made, I had two wires erected, each two miles in length, and running, as nearly as possible, the one north and south, and the other east and west. By means of simultaneous observations of the current in these two wires, I determined that there is a large daily variation in the direction of

the earth stream. At 5.0 p.m. the direction in India, according to my observations, is nearly south-east; during the night the direction becomes more and more southerly until sunrise, when it is nearly due south; it then moves westward by south until 9.0 a.m., when it attains its most westerly direction, say, 10° west of south. From 9.0 a.m. the direction moves eastward through south, which it passes about mid-day, and arrives again at its maximum easterly direction about 5.0 p.m.

The strength of the stream also varies during the day, though in a degree less marked than the variation in its direction. It attains a maximum at about 5.0 p.m., and a minimum at about 1 a.m. I may add that the variations in the direction of the stream appeared, from my observations, to be much more regular than the changes in its strength. It appears to be very probable that, while the variation in the direction of the stream follows its daily course with comparative regularity, the continual small variations we observe are due to ripples, as it were, on the surface of the stream, which ripples, in times of magnetic storms, expand into enormous waves.

I think these results may be of some importance in the development of the theories now being discussed.

In my former paper I expressed an opinion that the variations in the magnetometer were due to the direct action of earth currents on the suspended needle. In order to determine whether this were so or not, I had an instrument made which was described in a letter of mine to the Society, which letter has been published in the Society's Journal, but without any name.

The instrument was constructed as follows:—A large magnet was suspended under a small one, both being freely and independently suspended. The small needle was really a galvanometer needle, and was attached to the back of a small mirror in the usual way. Owing to the presence of the large magnet it was nearly in an astatic condition as regards the earth's directive force. Any changes in the earth's magnetism as a whole—that is, any changes in the direction of the lines of force in the earth—would affect both needles equally; but, owing to the astatic condition of the small magnet, the effect of any local deflecting cause, such as the

passage of earth currents through the earth below the apparatus, would be largely magnified on the small needle. This I found to be the case—that is, the movements of the small needle were very great; but, on comparing them with the motions of the magnetometer needle, they were not found to be merely enlargements of the variations in the magnetometer needle, neither were they altogether such as would be caused by the action of currents simultaneously observed in a line running north and south.

I think this is all that I have to say at present on the subject.

Professor AYRTON, in reply to a question from Sir Charles Bright, as to the exact explanation of the flow of water in the English Channel producing a current in the submarine cable there, remarked that the motion of a strip of water from England to France, across the vertical component of the earth's lines of force, would cause a difference of potentials to be set up at the two ends of the strip, which difference would be neutralised by a current flowing through any conductor joining the two ends of the strip. Now, there were many such conductors,—the whole bottom of the sea, for example, was such a conductor,—but one of them was any telegraph line, overhead or submarine, through which therefore a current would flow, and the direction of this current would depend on the direction of the tide, but not at all on whether the telegraph line was overhead or a submarine cable. Both ends of it, however, must be of course to earth, in order that the current could flow. In fact, the moving strip of water was just like the revolving armature in a dynamo machine, and the telegraph line like the external wire in which the electric lamp was placed.

Subsequently Professor Ayrton drew attention to the very full account of the last meeting that had appeared in the *Electrician*, of the discussion of Mr. Adams' paper, and especially to the statement of Professor Adams, that "Professor Rowland has also clearly shown that the theory which would account for the earth's magnetism, and for the changes in the earth's magnetic force, by supposing the earth to have a charge of electricity upon it, is utterly insufficient to produce the observed effects." This, Professor Ayrton thought not to be the case, since Professor Rowland had

shown nothing at all about a statical charge being unable to produce the *changes* in the earth's magnetism. He also criticised Professor Adams' statement, "Professors Ayrton and Perry's supposition as to electric charge on the earth would only account for $\frac{1}{8400000000}$ part of the terrestrial magnetism of the earth," as being a little misleading. If the maximum charge that could exist on the earth was only $\frac{1}{8400000000}$ part of that necessary to produce the total magnetism, then perhaps we might reject a statical charge as the explanation of the variations; but when it is remembered that the amount of statical charge necessary to produce $\frac{1}{1000}$ of the earth's magnetism will only cause a pressure the $\frac{1}{800}$ part of the normal atmospheric pressure, and is therefore quite a possible amount of charge to exist on the earth, while the normal lunar perturbations of terrestrial magnetism only required the $\frac{1}{12}$ part of even this small charge, and therefore would only produce a pressure equal to the $\frac{1}{12000000}$ of the atmospheric pressure, we could not, Professor Ayrton ventured to think, dismiss so lightly as Professor Adams had done the possibility that the diurnal variation on the earth's magnetism may be produced by perturbations in the distribution of a charge of electricity residing on the earth, and carried round with it by its rotation.

Professor Ayrton next drew attention to the fact that, whereas Professor Rowland was quite right in concluding that to produce the entire magnetism of the earth by a static electric charge revolving with it would, if all the charge resided on the surface, produce an impossible outward pressure, Professor Rowland was wrong in his statement that, "if the moon were electrified to a like potential, the force of repulsion would be greater than the gravitation attraction to the earth, and it would fly off through space," since the calculation made by Professor Perry and himself showed that whereas the mutual gravitation attraction of the earth and moon was $1·886 \times 10^{25}$ dynes, their repulsion, when both were charged to the required potential, would be only $1·401 \times 10^{24}$ dynes or about $\frac{1}{13}$ of the preceding.

Whatever might be the difficulties in explaining the cause of the earth's magnetism or the magnetic perturbations, Professor Ayrton thought there was reall no difficulty in explaining lunar

earth currents, the subject of the evening, from the fact that different points of the earth were at different distances from the moon, and therefore if the moon were charged, and the earth comparatively non-conducting, there would be a difference of electric potential between different points; and indeed calculation showed that an electro-motive force of one volt per mile along the line of steepest gradient would be caused if the moon had the easily allowable charge of 10,000 farads.

Professor Adams' idea of the lagging behind of portions of the earth's crust producing earth currents, was exceedingly suggestive, especially, as Professor Perry had already pointed out, in the connection between earthquakes and earth currents. The speaker believed he was himself the first to draw attention to this coincidence when in India—a coincidence that had since received frequent experimental confirmation, in some cases before an earthquake there having been greatly-increased earth currents even for a day or two. If Professor Adams' idea was right, that earth currents were caused by a lateral motion of the earth, a lagging behind of the surface of the earth roughly along a parallel of latitude, due to a terrestrial tide, then there would be an explanation of the connection of earthquakes and earth currents; for the greater subterranean pressure, which they might imagine anticipated the actual earthquake, would produce increased strain on the earth's crust, and hence, perhaps, increased earth currents.

But Professor Adams' suggestion would not explain the four maxima Mr. Adams considers he has experimentally arrived at. It would not explain why on the other side of the earth from the moon they had a positive current as well as on the side of the earth near the moon, nor did he see any explanation for that, except, possibly, some sort of local cause. It was easy to understand why there was a high tide on the other side of the earth when there was a high tide on this side of the earth, on account of the mass of water; but electricity, as far as we knew, had no mass, and therefore one would have expected two maxima, and not the four Mr. Adams considered his experiments pointed to.

The SECRETARY then read the following note on

THE ELECTRIC STORM OF JANUARY 31st, 1881.

By W. H. Preece.

Electric storms, which have been rare and quiet since 1872, have commenced to reappear. By *electric storms* I mean those *abnormal* earth currents which occasionally produce serious disturbances on telegraphic circuits. I do not in any way refer to those *normal* earth currents dealt with by Mr. Adams, which are always present more or less. The first storm occurred on August 11th and 12th of last year, but a second of much greater magnitude look place on January 31st last. They were both cosmical in their character, and evidences of their existence have been received from all quarters of the earth.

The storm of January 31st was first observed at 3 p.m., it reached a maximum at 6.40 p.m., and disappeared about 9 p.m. It was renewed about 11 p.m., and disappeared again about 1 a.m. the next morning. The currents acquired a strength that has never before in my experience been observed. At Llanfair, in Anglesey, on the London-Irish wires, they measured 41·4 milliwebers, while at Haverfordwest, on another Irish wire, they measured over 30 milliwebers. The observations made at the latter station were so carefully taken that I have plotted them out on a plan giving an excellent graphical representation of the duration, direction, and strength of the storm. The readings were taken upon a tangent galvanometer on a wire extending from Haverfordwest to Valentia, making earth at those two stations. The length of this circuit is 300 miles, and its resistance 4,000 ohms. The geographical distance between the two earths is 220 miles. The curve shows that the current continued steadily negative until 6 p.m., when it commenced to vary in strength considerably, acquiring at 6.34 a remarkable intensity, suddenly bounding over in the reverse direction in three minutes to the same intensity, viz., 30 millimetres. After varying for two minutes, its strength diminished considerably, and at 9 p.m. it disappeared. The maximum current shows that at that moment there was a difference of potentials between the two earths of 120 volts, or a fall of potential of 1 volt per 1·8 mile. The readings in Anglesey showed the fall of

potential between Anglesey and London to be exactly the same. Now, since working currents average about 10 milliwebers, it is evident that abnormal currents of such strength must have had a great disturbing influence on the telegraphic lines. In fact, at the period of maximum disturbance nearly all circuits were completely broken down, except where metallic loops could be resorted to.

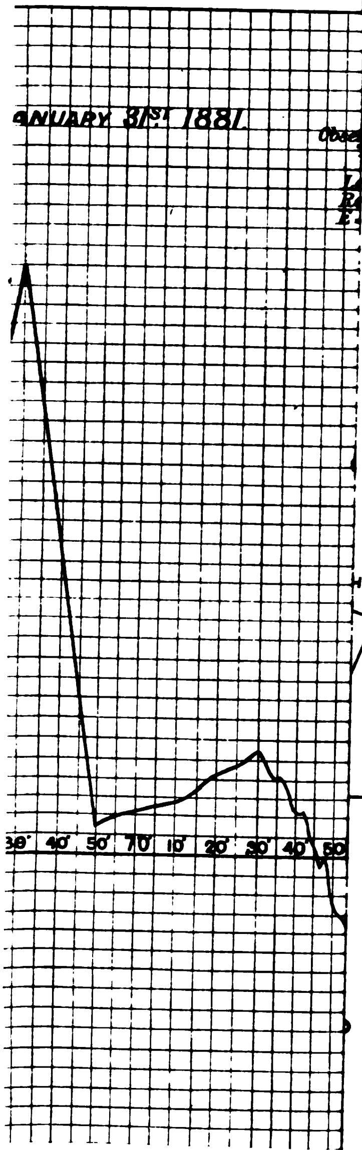
Some observations at Cardiff and Swansea enable us to form a fairly approximate idea of the distribution of the equipotential surfaces, and to deduce therefrom the line of maximum electromotive force. I have drawn them out on a small map. Moreover, calculation enables us to represent upon the same plan the distribution of potential as determined by the position of the sun with respect to the earth. It is remarkable how these lines coincide. I found precisely the same agreement on February 4th and August 7th, 1872, the last storms of any consequence. Moreover, their accuracy is confirmed by an observation taken at Lerwick, in the Shetlands, also shown on a map.

It is worthy of record that on this same day a disturbance of unusual and unprecedented magnitude was observed in the sun's photosphere. It is well known now that sun spots, auroræ boreales, magnetic disturbances, and earth currents are not only consentaneous, but that they obey precisely the same cycle, though authorities are not quite agreed upon the exact periodicity of this cycle. Secchi, in Rome, in 1872, proved that violent solar eruptions were coincident with violent magnetic disturbances and abnormal earth currents.

Taking these facts together there can be no reasonable doubt that these abnormal earth currents, or electric storms, are due, as I have previously pointed out, to some disturbance in the sun.

Mr. A. J. S. ADAMS apologised to Mr. Wollaston for his mistake in saying there was a cable under the Severn, and promised Mr. Varley the figures and the resistances of the lines which he desired, after which the meeting adjourned until March 10th.

Mr. C. F. VARLEY: I did not hear Mr. Ellis very well, but I think he has stated a good deal that I intended to speak of to-night.



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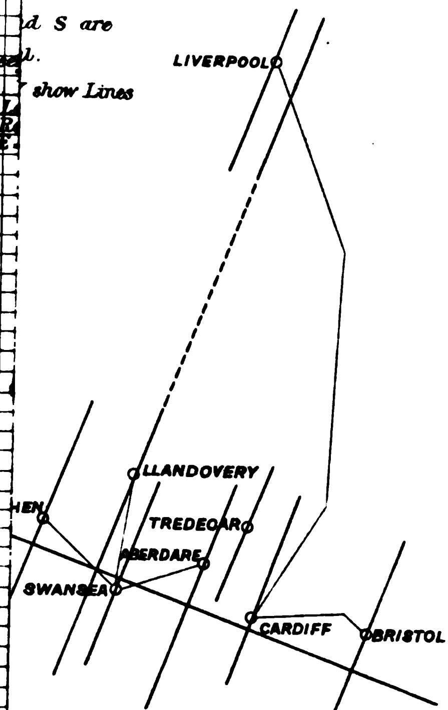
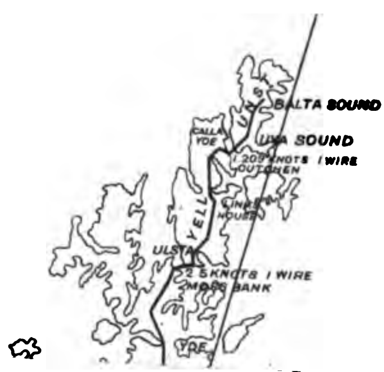




nearly neutral. Very often the neutral line would pass over,

has stated a good deal that I intended to speak of to-night.

First of all, I wish to compliment the author of the paper under discussion, and, if possible, to urge him to continue his observations, and, if he can possibly do so, give the exact figures as recorded, so that we may draw our own conclusions from them, as one can do from the figures given by the Astronomer-Royal every year.

The subject of earth currents and magnetic storms has occupied the attention of the Astronomer-Royal for a long time past, both with regard to the direction and strength of the current. I have had frequent communication with him upon the subject, and, in one of my communications to him in 1859, I pointed out that it was not possible to determine the actual direction in the earth by means of the currents through the land lines, because the direction and strength of the currents depended much upon the configuration of our island. I showed him clearly that sometimes it may be—as, for instance, with a line London to Ipswich, and a line London to Newcastle—that, when there was a positive current flowing from Ipswich to London and from Newcastle to London, there was actually a current passing in the opposite direction, from London to King's Lynn, which was situated between them. Now, on putting on an earth in the middle of that line, the cause was immediately found, for a current sprang suddenly in the wire which was nearly neutral beforehand, and it flew off to King's Lynn in one direction from the central point, and to London in the other direction, showing that the two currents were clashing. Lines whose two earth connections were parallel to the coast were, as a rule, neutral, while those perpendicular to the coast were, as a rule, much disturbed. I drew up a large number of diagrams and found that the neutral line was approximately nearly parallel to the coast. The line from Glasgow to Edinburgh, which was always a very much disturbed one, carried the electricity coming from the ocean on the one side and conducted it across to Edinburgh into the ocean again on the other side. Liverpool to Manchester and Manchester to Hull were very much disturbed lines. London to Ipswich was remarkable for its disturbance, and the lines from Bristol to London and London to Southampton were very often nearly neutral. Very often the neutral line would pass over, or

rather rotate across, these lines during the course of a disturbance, and the current would consequently change in direction. It therefore appeared that the land (in fact, the rock) which constitutes our island, being a much worse conductor than the sea-water, the current coming along the better conducting ocean, met with an obstruction in our island, and part of this current passed through our wires, producing powerful disturbances or currents. That this was the case was proved by comparing the strength of currents from the submarine cables with the currents through the land lines.

Our wires from London to Amsterdam were nearly in the same direction as the wires from London to Ipswich, and I frequently found that, by using a wire from London to make earth at Ipswich instead of at London, so that when Amsterdam was working to London the current had to come from Amsterdam to London and go back to Ipswich to go into the earth, the potential of the short London-Ipswich wire was almost identical with (it was a little less than) the potential from Amsterdam to London, confirming my previous observation. The same was observed, also, between Liverpool, Holyhead and Dublin, and therefore it would be impossible to determine the true direction of these currents by any observations made upon land; and the only really safe way to set to work about it would be, I think, to select isolated islands, such as some of the islands in the Pacific, and lay two cables across them at right angles, and attach to these cables earth-plates some distance out to sea, so as to make earth on each side, and then, by observation on those wires, one might very well determine accurately the direction of the current.

The Astronomer-Royal—I forget the year when it was done, but certainly it was done prior to my communicating with him upon the subject of earth currents (I think I began communicating with him about 1857)—had a wire running from Dartford to Greenwich, about ten miles in length, and another from Greenwich to Croydon, about eight miles in length. These wires were nearly at right angles one to the other. They were connected to the earth at Dartford, Greenwich, and Croydon. To these wires were joined two galvanometers carrying mirrors which reflected a

spot of light upon photographic paper, and thus produced the records which Mr. Ellis has produced. By this means the coincidence of the earth currents with the movements of the magnetometers was established fully as far back as 1859. The Astronomer-Royal has discussed the observations for the ten years from 1848 to 1857, and in the appendix to the volume for 1859 ("Greenwich Magnetical and Meteorological Observations," page ccxxxi.) will be found a table showing a distinct lunar influence. There is a rise and fall twice in each *lunar* day. He has discussed the observations for the five years ending in 1863. These will be found in the volume for 1867. The mean of these five years' observations corresponds remarkably with those of the former ten years.

This luno-diurnal irregularity for the period from 1848 to 1859, and from 1857 to 1863, shows two maxima and two minima in each *lunar* day.

	1848 to 1857.	1857 to 1863.
1st maximum	at 2 to 3 o'clock.	2 o'clock.
1st minimum	„ 7 „ 8 „	8 „
2nd maximum	„ 14 „	13 to 14 „
2nd minimum	„ 20 „	20 „

These mean observations were taken from those days which showed no great magnetic or electric storms.

I wish to draw especial attention to this, to show that the Astronomer-Royal was alive to the desirability of finding out if there were any connection between the motions of the heavenly bodies and these daily disturbances or currents, and he has found out, and has published, as far back as 1859, that there is a lunar variation coinciding with the ocean tidal variation—that is to say, a maximum is reached about 2 o'clock, a minimum about 8 o'clock, a maximum again at 14 o'clock, and a minimum again at 20 o'clock (see *Phil. Transactions*, 1870, page 215). The author of this paper (Mr. Adams) drew attention to what he thought an anomaly in his results, viz., that in each lunar day there were four maxima, two maxima of positive and two negative. That is apparent, but not real. When we discuss ocean

tidal action, we measure the tide from the bottom of one tidal wave to the top of the other. Now, Mr. Adams' zero line corresponds with the mean level of the ocean were there no sun and moon, and therefore there are but two tidal waves a day, which should be reckoned from the maximum negative to the maximum positive, as is done from ebb tide to flood tide. These are found to coincide exactly with the tides which are produced principally by the moon. But we all know that there are other tides besides those of the moon. The sun gives a very considerable tide; Jupiter and Venus each give a sensible tide. If Mr. Adams were to carry on his observations for a series of years, I have no doubt that he would find electric spring tides and neap tides.

The observations made by Mr. Ellis were very much to the point indeed, but I wish to draw especial attention to the results of the Astronomer-Royal, because the reports are in the library of this Society, and, as the data extend over a period of many years, the figures are very reliable.

We all know that the moon produces a tidal motion in the ocean. It is also known that it tends to produce one in the solid body of the earth, but the quantity is so small, on account of the prodigious hardness of the solid interior of the world, that no rocking of the surface of the earth has been observed, notwithstanding the delicacy of the splendid apparatus employed at the different observatories. Besides the ocean, there is another fluid—the atmosphere. The atmosphere ought to be affected like the ocean, and at places near the equator a distinct barometric tide is visible. The moon when on the meridian causes that part of the ocean next to it to rise towards it, being pulled more powerfully than the solid body of the world, and this again more powerfully than the ocean at the nadir; the ocean, therefore, takes an elliptical form. In the case of the atmosphere, this elliptical shape will take place to a much greater extent than in the ocean; oxygen being magnetic, this will cause a lunar diurnal variation. In a paper which I read before the Royal Society about ten or twelve years ago, I drew attention to the diminished resistance of the atmosphere, when attenuated, to the conduction of electricity, and ascertained the law which governed the flow of electricity through rarified media. At

an elevation of 40 miles or more above the surface of the earth, we should find that the atmosphere conducted with many hundred times more facility than near the surface of the earth. Now, if we imagine (as we all know is the case) this conductor to be highly charged with positive electricity, we shall have, by means of this atmospheric tide, the positive electricity lifted up from the surface of the earth, and consequently there will be a rush of positive electricity along the earth's surface to that point. As this tidal wave is shifted over to another place, as cccurs each twelve lunar hours, the electricity will flow from one point to another, and thus probably produce the irregularities observed.

In the report of the Submarine Telegraph Committee, appointed by the Privy Council and the Atlantic Telegraph Company, page 152, I mentioned that the earth currents in 1859 were very powerful. One was so strong on the line between London and Ipswich, 69½ miles, that it required 140 Daniell cells to balance it, showing a difference of potential of 2 Daniell cells to the mile. In the same book, pages 504 to 511, inclusive, will be found some of the letters which passed between the Astronomer-Royal and myself upon the subject, also charts of mine showing the neutral lines.

In South Devon, in 1847, I found in a circuit of three miles a current that required 22 zinc copper cells to balance it, or somewhere about six or seven volts to the mile.

The Ninety-seventh Ordinary General Meeting of the Society was held on Thursday evening, 10th March, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster —Professor G. C. FOSTER, F.R.S., President, in the Chair.

The minutes of the last meeting were read and confirmed.

The names of new candidates for election were announced and suspended, and it was announced that the Council had transferred the following Associates to the class of Members:—

St. George Lane Fox,
Joseph Howe.

The PRESIDENT: Before proceeding to the special business of the evening, I think it desirable to call the attention of members to a matter of which they have, no doubt, more or less definitely heard already—the intention to hold an Exhibition of Electrical Appliances in Paris during the forthcoming summer. It was hoped that the English Government, like the Governments of most of the important countries on the Continent, would have appointed a Special Commissioner to look after the interests of the exhibitors of their own country, and, in consequence, no action has hitherto been taken by this Society or any other body in the matter. But I am sorry to say that our Government have refused to make any such appointment, and there seems to be no reasonable hope of inducing them to reverse their decision. The Council of the Society have therefore taken upon themselves to do all that can be done in the short time available. It is desirable that the fact that the exhibition is about to be held should be made known as widely as possible. The Council will try to secure this, and undertake to take charge of any applications for space with which they may be entrusted, and transmit them to the General Commissioner for the Exhibition; in fact, to act as far as possible as intermediary between the French executive and British exhibitors. I state this that members may assist in distributing the information, by communicating the fact to their friends. The latest date up to which applications for space can be received is the 31st March.

We will now resume the discussion on Mr. Adams' paper on earth currents. As so much time has already been given to this matter, I would request that further remarks be as brief as possible.

Mr. WILLOUGHBY SMITH : As it has been suggested to me that my silence might be taken as an admission of the correctness of Mr. Wollaston's observations on the experiments with the two wires laid across the Thames, mentioned by me at our last meeting, I may perhaps be allowed to make a few further remarks.

If I have rightly understood the subject, the wire Mr. Adams used for his experiments extended from London to Cardiff, and a very small part of the whole length passed either under or over the river Severn. Both ends of the wire were to earth, but between the London end and the earth was placed a galvanometer, the movements of the needle of which were recorded by Mr. Adams as being the effects of earth currents; but Mr. Wollaston says the effect of the tide passing either over or under that portion of the wire crossing the Severn would produce the same results.

Now, supposing the correctness of Mr. Wollaston's statement, the effect could only have been one of induction; consequently, in the experiment with the two wires laid across the Thames, they were placed under the most favourable conditions for detecting induced currents. Had both ends of each wire been put to earth, as Mr. Wollaston says they ought, a current due to polarisation of the earth plates would have been obtained, and nothing more; and, independent of the interference of the polarising current, the conditions would then have been most unfavourable for ascertaining the correctness of Mr. Wollaston's assertion. The inductive effect would be in direct proportion to the tension of the influencing body and the length of the wire exposed, consequently it is not difficult to prove by actual experiment what the result would be under certain conditions. Suppose 40 feet of insulated wire laid in a straight line, with one end to earth through a very sensitive galvanometer, the other end being insulated; and a similar wire, one end of which is insulated, and the other end connected to one terminal of a battery of 500 cells, the other terminal being to earth; and suppose the charged wire is brought sufficiently near to the uncharged one, then a slight momentary deflection would be

observable on the galvanometer, and if the charged wire be removed, a similar momentary deflection would be produced, but in the opposite direction. From this I learn that, had either one of the wires laid across the Thames been influenced by any external body of a higher potential, it would have been immediately detected; and had the potential of such a body been equal to 500 volts, then the inductive effect would only equal the $\frac{1}{25000}$ of such potential. Now, under the same conditions, but with both ends of the test-wire to earth, no effect but the current due to polarisation would be observed. When in 1866 the Atlantic cable was being shipped on board the "Great Eastern," then lying in the Medway, a wire was immersed between the ship and the shore for practice in my then new system of testing and working through cables during submersion; and, although that wire was at right angles to the strong tide of the Medway for two months, no sign of any external electrical disturbance was observed. If time permitted, many similar cases might be cited.

Mr. A. J. S. ADAMS, in reply, said: The discussion which has followed the reading of my paper has been most gratifying to me, and I believe useful. I must ask your forbearance, however, should I not reply to all the criticisms that have been offered, because, in some cases, I have had a difficulty in discerning whether remarks have had particular references to the earth current or to current storms.

There is little doubt but that, in a minor degree, there is also a solar variation of the earth current, and it is a subject I have for some time been investigating; probably, however, unless the apparatus used have sufficient range and sensitiveness, the different variations, both solar and lunar, will become merged, so that the result will be more or less indistinct as regards solar or lunar causation, especially during that part of the lunation when the solar and lunar influences are more or less parallel.

As regards the discrepancy between the observed and the theoretical position of maximum magnetic effect upon the surface of the globe, I think it hardly correct to assume that, if all disturbing influences were removed, the position of any such maximum effect would remain unchanged: there are doubtless very con-

siderable modifying influences present in and upon the earth. Nor should we judge of the probable magnetic force producible by these current systems, from the assumed or calculated quantitative charge of the earth or moon alone: the *speed* with which the electricity circulates must be included in the calculation.

The effects of electric storms were eliminated from the observations I have submitted, but such storms only materially affected the normal curve for short periods upon the 2nd, 7th, and 11th of April, 1879, and even then the greatest strength of the storm currents did not exceed twice that of the maximum earth current.

I was pleased to find corroboration of the lunar progression in the interesting curves furnished by Mr. H. C. Saunders; but, if possible, such observation results should be compared with the position of the moon and sun, rather than with the time of high water.

I have received from Mr. Preece some results of earth-current observations taken at Abdelah, in Persia, which, although very incomplete, clearly indicate the usual four maxima and zeros; and I am also indebted to the kindness of Mr. Preece for the sight of a letter from Mr. James Graves, of Valentia, of which the following is an extract. Mr. J. Graves says: "It may be of interest to you to know that I took observations of polarity and strength of earth currents from March 18th to April 5th, 1873, for the purpose of tracing some connection between them and the *moon's position daily;* and I now enclose the sheet I then drew up, with belts across where the negative readings preponderated, and which are generally pretty equidistant, and show a retrograde motion (as to time) of about one hour per diem, so that in a lunar month the motion of the negative belt crosses the 24 hours. . . . Mr. Alexander Adams seems to have hit upon the same result by his study of the earth current on land lines." And in a note attached to his diagram, Mr. Graves adds: "Of the 568 nautical miles of cable upon which these observations were made, a very small proportion of it would be under the influence of ocean tidal flow and ebb—perhaps, however, sufficient to give a record as per Faraday's researches." I beg to hand the President Mr. Graves' results, and it will be seen how apparent the lunar progression is.

Mr. Wollaston's remarks recalled to my own mind an incident which presented somewhat similar features to those he described.

I think it was in 1868 that Mr. J. B. Chapman—of the Post Office—and myself spent some time in endeavouring to localise a fault in one of the Irish Sea cables. The variations in resistance and polarisation-current were so peculiar that the then Engineer-in-Chief directed that tests should be taken night and day. The variatious followed absolutely the run of the tide; and, more curiously still, every high wave that passed up or down channel was clearly indicated upon the galvanometer. Those were tidal currents with a vengeance! but it transpired that during a gale the grapnel of a buoy in the North Irish Sea had slipped and was dragged down channel until it caught hold of a cable in the South Irish Sea, and in due time produced a fault which varied with the strain put upon the grapnel by the rise and fall of the buoy. I have little doubt but that in shallow water the tidal flow would have a somewhat similar effect upon any very minute fault in a submarine cable, both by reason of variation in temperature, strain, and increased weight of water. Possibly, however, the currents related by Mr. Wollaston were really earth currents, such as have been described in my paper. Theoretically, Professor Ayrton is probably correct as regards the electrical effect of tidal waters passing across a submerged wire, but practically no appreciable current would result therefrom: certainly the earth currents referred to in my paper would not be due to such action.

The four maxima and zeros, which appear to have been a source of difficulty in the discussion, are practical facts. Each maximum, by whatever term we may describe it, is a distinct rise and subsidence of current in a given direction, and we should not, I think, ignore or seek to explain away the facts because they will not readily coincide with theory, or with the effect of static disturbance as we may happen to be acquainted with it.

Nor does it appear necessary to consider that the earth-current effect is due solely to direct static action, although, as secondary or as primary influences, neither the thermal theory nor the idea of a tidal motion of the earth's crust in any way assists to a solution. There are, however, other possibilities which may be worthy of con-

sideration. For instance, if the earth current be due to difference of potential upon the earth's surface, then, presumably, the longer the wire in a given direction, the stronger should be the current, other things being equal: but, as will be seen from the following results, experiment shows that the length of a wire has little influence upon the strength of the earth current; whilst, upon the other hand, the introduction of a very moderate resistance is sufficient to reduce its strength.

EARTH CURRENT OBSERVATIONS UPON WIRES DIFFERING IN LENGTH, BUT EXTENDING IN THE SAME DIRECTION, AND HAVING THE SAME RESISTANCE.

Wire extending from London to	Distance in Miles.	Earth Current Readings.						Weather.
		1	2	3	4	5	Mean.	
Nottingham ...	128	+10	— 5	—10	—10	—20	11	Fine.
Sheffield	165	+12	— 5	— 5	— 5	—13	8	"
Leeds	225	+ 8	— 9	— 9	—12	— 9	9·4	Dull.
Glasgow	420	+ 9	— 8	— 7	—12	—20	11·2	Dry.
Wolverton	52	—18	—12	— 7	— 8	—15	12	Fine.
Rugby	82	— 6	—18	—12	— 7	—16	11·8	"
Manchester... ...	190	—13	—10	—10	—11	—12	11·2	Dull.

Such results suggest to my mind, first, that the current effect is due to the movement of a body of static electricity, for it appears to lack in great measure the element of electro-motive force; and, secondly, that that movement is mechanical. I mean by mechanical, that, if the earth and her electricity travel in equal ratio, no current effect will exhibit itself upon a wire stretched over the former, but if the moon, by inductive influence, causes a retardation or dragging of the electricity as it passes under her at the major and minor poles, relative speeds are set up between the earth and her electricity, and current effects *in a given direction* are the result, the strength of which will be *proportionate to the difference of those speeds* rather than to the earth or moon's specific charge.

Likewise, may not an acceleration of the earth's electricity at the electric circle result as compensation for that retardation, so

that currents are produced in the opposite direction to the former? Under such conditions the observed four maxima and four zeros would become a necessary consequence.

But whatever the cause, these four maxima and zeros are most interesting features, and may yet perhaps throw light upon the precise causation and action of oceanic tides.

Some remarks were made in reference to the lagging of the tidal phases behind the moon. To me this feature is a curious and difficult problem; so much so that I am obliged to ask myself whether this retardation may not be in reality *acceleration*—excuse the paradox;—but what I mean is this:—

Suppose a disturbance to have reached the earth at the instant of leaving the moon, may not the resultant effect of that disturbance be carried forward by the earth in her rotation in consequence of the superior attraction or unity existing between the earth and her electricity, so that a given point upon the earth's surface would have to pass the moon, as regards time, before it reached the full effect of her influence?

In conclusion, I beg to thank you for the very kind attention bestowed upon my paper.

The PRESIDENT: I am sure I shall have the approval of the meeting in proposing a cordial vote of thanks to Mr. Adams for his paper, which has led to an extremely instructive and interesting discussion. The subject of the paper is a very complicated one. It is obvious, I think, that in these earth currents there are both local and general effects—effects which we can only hope to have much light thrown upon by observations extending over very considerable portions of the earth's surface. The problem becomes more difficult from the mixing up of effects attributable to local causes with those affecting larger surfaces, therefore all the attention possible should be given to the matter; and we may hope that by observations such as those made by Mr. Adams, and by discussions such as that which has just concluded, our knowledge may be greatly extended.

A hearty vote of thanks was then accorded to Mr. Adams, and also to Mr. Preece for his supplementary note upon the electric storm of January 31st.

Lieutenant P. CARDEW, R.E.: Perhaps I may be permitted to mention a curious incident which touches upon the discussion just closed. Lately I have had a bare wire, 15 miles long, laid out on the ground for telephone experiments, not insulated in any manner, but lying on the ground. On Tuesday last (March 8th), at three o'clock, a peculiar current came on the telephones, a current quite distinct from that produced by a battery (which is scratchy and irregular), but a sort of rush through the telephone, quite regular. At the moment a storm was passing over and down the line along which the wire was laid. The effect was perhaps due to the charge of this storm, but I do not pretend to explain the cause.

Mr. H. C. FORDE: Fifteen miles of wire straight out?

Lieutenant P. CARDEW: In a loop, with one turn in it.

The following paper was then read:—

ON THE APPLICATION OF DYNAMO-ELECTRIC MACHINES TO RAILWAY ROLLING STOCK.

By LIEUT. P. CARDEW, R.E., Associate.

Dr. Siemens has practically demonstrated the possibility of propelling trains by means of the electric current, generated by fixed machines at intervals, and transmitted by the rails to an electro-motor taking the place of an engine. This system has obvious advantages in particular conditions to which it will undoubtedly be applied, such as the transit through long tunnels, but it will never supersede the steam locomotive for ordinary working.

It appears to me, however, that dynamo-electric motors might possibly be advantageously applied to the ordinary railway locomotives and carriages—

1, As an auxiliary means of transmitting the pull of the engine to the carriages, and thereby lessening the wear and tear on the rails, and the slip of the driving wheels;

2, As a simple and efficient brake.

As I have seen no proposal as yet for such an application of these machines, I venture to hope that a short paper on the subject may be thought worthy of insertion in the *Journal of the Society of Telegraph Engineers and of Electricians.*

I propose fixing a generating machine of considerable power on the engine, and motor or receiving machines on a certain proportion of the carriages—the former to be driven from the shaft of the driving wheel of the engine by gearing at a high velocity; while the armatures, or revolving magnets, of each of the latter would be fixed on the axle of a pair of carriage wheels, turning with it.

The generating machine might be a simple form of Gramme machine, in which the number of convolutions of wire both in the revolving and stationary coils should be considerably reduced from that suitable for electric light machines; while the size of the wire should be proportionately increased, since a high electro-motive force will not be necessary, as there is no arc to be maintained, while the current will be unusually large.

The receiving machines might be of a simpler nature still, and perhaps especially designed as motors, for which purpose it is generally allowed that the existing dynamo-electric machines are not entirely satisfactory, a considerable amount of the magnetic pull being badly disposed.

A point of primary importance for such machines is that they should require a minimum of attention. The sum of the resistances of these machines should equal that of the generating machine.

The connections would be formed by copper-rope leads, of large section, fixed along the bed of each carriage, the ends projecting and being coupled to each other when the train is made up. The tracing would be as follows:—From one brush of the generator through its own coils, thence through all the standing coils of the receivers to one side of a short-circuiting switch in the guard's van; from this a lead back to one side of a similar switch on the engine; thence through the revolving coils of the receivers to the other side of the switch in the guard's van, which is connected to the other side of the switch on the engine, and thence to the other brush of the generator. Thus it will be seen that when the switches are open the whole are in continuous circuit; but the receiving machines, which, as will be seen, necessarily revolve at the same rate as the carriage wheels, are so arranged that the electro-motive force produced by them is in opposition to that of

the generator, the dimensions and speed of the latter being such as to produce a stronger E.M.F. than the whole E.M.F. in opposition, or, in other words, to tend to turn the receivers faster.

When the train has to be stopped, after shutting off steam, the engine-driver or the guard, or both of them, would put down their short-circuiting switches. The effect of this on the generator will be to remove the opposing E.M.F., and at the same time reduce the resistance in its circuit, causing a large increase of current, and increasing greatly the work it is doing. At the same time the magnetic field in which the armatures of the receiving machines revolve will be strengthened, while the current produced by them only passes through the short circuits and two leads in divided circuit. The energy of this large current will therefore almost entirely be usefully spent in stopping the train. As the speed of the train is reduced, the current is lessened, and thus injurious heating of the large section wire employed is avoided.

It will be in the armatures of the receivers that large section wire will be most essential, since they are connected together in "tension," and absolutely short-circuited. The contacts with these armatures would probably be best accomplished by rollers, in place of the brushes generally employed. The stationary coils of the receivers have to be connected separately from their revolving coils, because they are necessarily turned in a "reversed" direction, so that they would produce no current on short circuit if their magnetism were not maintained by the current from the generator.

For "backing" the train the circuit should be broken, which could be effected automatically.

I think it will be admitted that an efficient brake-power could be provided by this means, and that its application would be simple and certain in operation. The most obvious objection to it is the expense of the machinery, which would have to be very powerful. But, as I have already pointed out, these machines would not have to maintain an electric arc, for which a comparatively high electro-motive force is necessary; hence the number of convolutions, and consequently the expense of winding would be considerably diminished.

Power would be obtained by reducing the total electrical resis-

tance to a minimum, perhaps not exceeding ·1 ohm, all the parts being made massive, the wire of very large section, and all the electrical joints in the machines and conductors, except the connection between carriages, being solidly soldered in place of relying on pressure for contact.

The latter point is one which I think has hardly been sufficiently attended to in the low resistance machines at present used. With a sensitive quantity detecter, deflections can be obtained between the two sides of nearly every joint in a dynamo that has been at work some time, and I have known a seemingly tight joint show a difference of potential of 4 volts, indicating in a machine of an electro-motive force 60 and a total resistance in circuit of 1 ohm, a resistance of $\frac{1}{15}$ ohm.

The screw connections between carriages might be made on the same principle as the connections in fire-engine hose. The commutators and brushes would have to be of such form as to stand considerable wear, and avoid sparking, especially those of the receiving machines, which would probably be difficult to get at.

The designing of the machines would require some preliminary experiments, which, however, are not likely to be carried out. Of course arrangements could be easily made to shunt a sufficient current on to a light circuit, so as to show a powerful light when running through stations in foggy weather. I hope the idea thus roughly sketched may possess some interest, even if it should be thought impracticable. Certainly, the communication of motion from the engines to the carriages by some other means than the friction between the driving wheels and the rails, and an efficient brake-power which can be certainly and easily applied instantaneously, are in themselves desiderata in railway economy.

It is perhaps worth notice that, in this application of the dynamo principle as a brake, the whole energy of the current is usefully expended, the work done in the external circuit being practically nil, while that done in all the machines tends to stop the train.

Mr. ALEXANDER SIEMENS: The proposed application of electricity to railway brakes is certainly interesting, but I think, in

the form in which it has been put in the paper, that it is hardly practicable. It would be difficult to wind the helix or revolving armatures on railway carriage axles. I should not like, either, to rely upon a dynamo-machine running a long time short-circuited. The cost of the proposed thick copper wire would be very serious, seeing that four wires would be required. Several attempts have been made in this direction, but the great objection has always been that the distance from which a magnet works is rather short.

Mr. C. E. SPAGNOLETTI: What Lieutenant Cardew has brought before us is very interesting, but there are, I fear, too many difficulties in the way of its adoption to allow it to be brought into general use. Having four additional connections between the carriages, requiring to be carefully made, would be a great difficulty in the practical working of a railway, especially at junctions where trains are often broken up and re-formed: the necessity for adding to or taking from a train carriages and other vehicles, would be another great obstacle in the way of the adoption of the system, especially in its present form. As to the slipping of the wheels, the only ones which slip in a train are the driving wheels of the engine, upon which is the whole weight of the train on starting, and until the train is into speed the assistance of the electrical power as proposed would not be given.

Mr. CROMPTON: It has long been a matter of astonishment to me that a good electric brake has not been brought forward. Any one who has noticed the extraordinary "pulling-up" effect on the driving-engine when the circuit of a dynamo-electric machine is closed, must have felt that a brake-power existed which ought to be utilised for railway purposes. I think Mr. Spagnoletti overrates the difficulty of the fittings. Railway companies already have air-tight connections between their carriages for air brakes, and I consider that the connections for an electric brake would not be more difficult to manage. There is something to be done in this direction, and it is my firm conviction that something *will* be done in it before many months have passed. The difficulty as to winding the bobbins on the carriage axles, mentioned by Mr. A. Siemens, can be got over by adopting a suitable form of armature which already exists. Lieutenant Cardew will get into difficulties

by reducing the number of the armature segments: he will have much sparking at the brushes, etc. To avoid sparks and severe wear of the collecting cylinder, it is advisable that the current be distributed over a large number of segments, and the adoption of a thick wire will not in any way compensate for a reduction in the number of segments. In my opinion the brake suggestion can be, and will be carried out, and the only wonder is that the patent list does not, up to the present time, contain a greater number of references to this subject.

Mr. G. K. WINTER: I believe the idea of short-circuiting a dynamo-electric machine, to act as a brake to a train, has been patented, but I do not remember by whom. By using V-shaped hooks for the ordinary coupling chains of a train, and soldering the leading wires to these hooks, I have not found it difficult to ensure good electric connection, to be used for signalling purposes, being easily made throughout the whole train when it is coupled up.

Professor AYRTON mentioned that the patent Mr. Winter referred to was, if he remembered rightly, one of Rapieff's.

Professor HUGHES: There is an excellent brake used in France, called "Achard's" brake,—not, however, depending upon the employment of a dynamo machine,—which is now undergoing trial against the Westinghouse.

Mr. J. MUNRO: The dynamo machine has been applied to the brake referred to by Professor Hughes.

Mr. H. R. KEMPE: One point seems to me to have been overlooked by brakes in dynamo machines being short-circuited—*i.e.*, when a train is brought up in that manner, it is obvious that the current will become weaker and weaker as the train stops, and consequently the brake will slacken off. In non-electrical brakes the reverse process is observed.

Professor AYRTON: Is not Mr. Kempe forgetting that it has been distinctly proved by Captain Dalton that the proper method of applying brake-power to a train is to apply considerable force at at first, and then rapidly diminish it as the speed of the train diminishes—in fact, always to keep the retarding force less than is necessary to skid the wheels? The diminished braking power of wheels when skidded, arising, probably, from kinetic friction

being less than static friction, is so fully realised on the Metropolitan Railway, for example, that you may see in the brake-vans notices containing strict injunctions against skidding. Now, an electric brake would act most powerfully when the speed was greatest, and would not act at all if the wheels were skidded: hence an electric brake fulfils most perfectly the conditions arrived at by Captain Galton, as necessary to be fulfilled to stop a train in the most efficient way. I quite agree with Mr. Crompton that the apparent difficulty of connecting up the carriages electrically is purely imaginary. The successful introduction of vacuum and air-pressure brakes has proved that there is no real difficulty in having such connections properly made, if only proper couplings are devised. Lieutenant Cardew has, I think, complicated his very ingenious system by using so many wires; and, were there not another paper waiting to be read, I should like to indicate somewhat fully another method that has occurred to me, and by means of which the same results can, I think, be more simply attained.

The PRESIDENT: There is another action besides the one that Lieutenant Cardew has made the subject of his paper, available for a brake for machinery—that is, the effect due to the induced currents in a conductor moving near a magnet. The stopping of a copper disk moving between the poles of a magnet is a very familiar experiment. If the disk is at all thick, even a very moderate magnetic power causes a very rapid stoppage. That is an instance of a perfectly smooth brake which would follow precisely the conditions which Professor Ayrton has mentioned; there could not be any possible skidding. It is a question whether sufficient power could be got in this way, but, if we could have a powerful electro-magnet in three or four carriages, we might get a very great brake-power. It would not, of course, fulfil the other part of Lieutenant Cardew's plan of distributing the driving power at various parts of the train, instead of having it all concentrated in the engine.

Professor AYRTON: I am not sure that the idea of using a dynamo machine is not the mistake in the arrangement, and that far better results could be obtained by using magneto-electric machines. One of the great difficulties in dynamo machines is the

necessity of having a strong current to magnetise the field magnets, and for such strong current there must consequently be thick wires to prevent fusing; but with magneto-electric machines with permanent field magnets there is no necessity for a strong current, and in such a case we can have the transmisson of a large amount of power with a weak current, or, consequently, with only a small waste of power. To attain this result, the armatures both of the generator and motor must be moving very rapidly. With magneto-electric machines, Lieutenant Cardew's connection could be very much simplified, since all that would be necessary would be merely to make one reversal of the connections between the generator on the engine and the motors under the carriages, in order to convert the moving power into a brake-power; whereas, with Lieutenant Cardew's arrangements, the current flowing through the field magnets has to be kept constant in direction, and only that flowing through the revolving armatures reversed, and hence his connections appear rather complicated. And, as I have already mentioned, far greater efficiency can be obtained with magneto-electric machines than with dynamo-electric ones.

Up to the summer of 1879 there seemed to be a curious misconception existing in the minds of even distinguished electricians, regarding the theoretical considerations of the efficiency of electric transmission of power. There are two distinct questions to consider: Do you want a motor to give out work rapidly? or do you want the arrangement to transmit power most economically? The two questions are quite easy to solve, and lead to quite distinct answers. Let E be the electro-motive force of the generator, e the back electro-motive force of the motor, and r the total resistance in circuit, then the work put into the generator is

$$\frac{E - e}{r} E,$$

and the work given out by the motor is

$$\frac{E - e}{r} e$$

If now we want the latter to be a maximum—that is, if we want work to be given out most rapidly, independently of the work put into the generator, then e must equal $\frac{1}{2}$ E; and, as the efficiency of

the arrangement is always the ratio of the two fractions, or $\frac{E}{e}$, the maximum efficiency when the motor is giving out work most rapidly is ½. And this was the result that had been arrived at, and which it was erroneously assumed would *always* apply. But, if we desire to transmit work most economically, we must make not $\frac{E-e}{r}\,e$ a maximum, but simply $\frac{E}{e}$, and the answer to this is, of course, make $E = e$. Then the current flowing through the wires connecting the generator and the motor will be very small, and, consequently, unless magneto-electric machines or dynamo-electric with separate exciters are employed, the gain in efficiency will be at the expense of the amount of the work transmitted, since the field magnets will not be excited. But when it is so arranged that the strength of the inducing magnetic field in both generator and motor is independent of the strength of the current which conveys the power from one to the other, then E may be made nearly equal to e by putting so light a load on the motor as to allow it to run nearly as fast as the generator, and then the waste of power which depends on $E - e$ will be nearly nought. But if the actual speeds of each machine be very high, E and e will be very great, so that, although $E - e$ is small, the value of $\frac{E-e}{r}\,E$ and of $\frac{E-e}{r}\,e$ may be made as great as we like—that is, the waste may be diminished without diminishing the power actually transmitted. It was this consideration that led Professor Perry and myself to anticipate in 1879 that, so far from ½ being, as was then stated in all publications on this subject, the limit of the efficiency, ⅘ or more could be attained—a result which has, I am glad to say, since been experimentally corroborated.

The water analogy may perhaps not only assist in forming a clear conception of this question, but actually shows that in this case perfectly similar considerations are applicable both for electric and for fluid transmission of power. Supposing we have a force-pump forcing water up a height into one end of a pipe, and that the water falling out at the other end works a turbine, then clearly the work expended at one end depends, not only on the

amount of water pumped up, but on the height to which it is raised, and the work given out by the water at the other end of the pipe depends partly on the amount of water falling and partly on the height through which it falls. The waste of power in the pipe depends, for a given pipe, only on the amount of water passing, which again depends on the difference of level. If, then, we make this difference of level very small, we shall diminish the flow, and also the waste; but, by raising the pipe bodily up higher and higher, we can, for the same very small current and absolutely small waste, make the work put into the water at one end and the work given out by it at the other as great as we like. Raising the pipe very high up in the air is of course a mechanical difficulty, but the same result can be of course arrived at if we make a very little water flow through a pipe on the ground at very great pressure. Great strength of pipe, which would be necessary if the water were under great pressure, is, however, far more difficult to attain than very good insulation for our wires, which would be necessary if the electro-motive force of both generator and motor were very large.

Lieutenant Cardew, R.E.: I must express my thanks to Professor Ayrton for the able remarks he has made upon my paper. I had thought of magneto machines, and am aware of their superior efficiency, but I hardly think that the magnets would be sufficiently strong as compared with the magnetism produced by large dynamo-electric machines, and the continual vibration might produce an injurious effect on their permanence.

I am also obliged to Mr. Crompton. As to the short-circuiting, it does not make the least difference whether each separate machine is short-circuited, or the whole lot. It is the same thing, as there is no appreciable resistance except in the coils. If the machines were separately short-circuited, no better effect would be produced than is at present obtained. As to the slipping of the wheels, all the work done in the generating machine (I mean that portion which is usefully expended, which I have noted as 90 per cent. of 45 per cent.) is transferred to the carriages without being transferred by the friction of the driving wheels to the rails, which we know causes destruction of the rails.

Professor AYRTON: Is that 45 per cent. the result of practical experiment?

Lieutenant CARDEW: Yes.*

A hearty vote of thanks was then accorded to Lieutenant Cardew for his paper.

The following paper was then read:—

ON THE INTERFERENCE WITH THE PROCESSES OF MANUFACTURE OF WOOL AND HAIR, ARISING FROM THE DEVELOPMENT OF ELECTRICITY DURING SPINNING, WITH A DESCRIPTION OF APPARATUS APPLIED TO OBVIATE IT.

By E. B. BRIGHT, C.E., Member.

The attention of members of this Society is usually directed to the best means of producing and utilising electricity, but in the present paper I have to bring before your notice apparatus devised to get rid of electricity developed during the spinning of wool, which has proved an embarrassment in the processes of manufacture.

This trouble in working is especially experienced in connection with alpaca, mohair, and the high class or (so-called) "lustre" wools, both during the preliminary "combing" and when the yarn is being drawn down by successive operations to the fine numbers. The friction, partly of the machinery, but mostly of

* This question was asked me just as I was leaving in a hurry to catch a train, and I am afraid that my answer may have misled Professor Ayrton and others into the belief that I had experimented in this way with dynamo machines and measured this amount of efficiency.

An efficiency of 90 per cent. has been obtained here and by other experimenters with generating machines, and, assuming the motors to be equally efficient and of equal resistance to the generator, this would give 90 per cent. of 45 per cent., or about 40 per cent. of the work consumed transferred to the carriages.

Fontaine, in his work on the electric light, states that 61 per cent. of the expended work has been practically obtained from the motor. This can only have been effected by employing a motor of considerably higher resistance than the generator.

the fine filaments of wool one upon another, developes a considerable amount of electricity, causing the yarn to become (using the technical term) "stickleback."

In the diagram to which I now call attention, I have endeavoured to portray on an enlarged scale the "stickleback" condition that the yarn assumes. Every hair, being charged with electricity, both within and without, has the tendency to stand on end away from the neighbouring hairs, in order to discharge itself. It clutches everything in its passage through the "roving" machinery, and by lapping upon the "drawing" rollers causes constant breaks of the yarn, and giving much trouble to the operatives.

It is naturally much aggravated by dry weather, and manufacturers have been sometimes obliged to stop working for days together.

In climates where the atmosphere is generally drier than in this country, the phenomenon is of course more frequently met with.

I am informed that some years ago an equipment of spinning machinery for these fine wools was sent out to the United States from Bradford, and a factory was fitted up with it; and worked by Yorkshire operatives, also exported. It was found, however, that spinning could only be carried on for a few months in the year, and that principally during the periods of thaw, or in very damp weather; and although steam was admitted into the factory rooms with the view of mitigating the evil, while nearly stifling the workpeople the manufacture had to be given up. It is also stated to me that, owing to this difficulty in dry climates, large quantities of yarn and thread are sent from our Yorkshire factories to various parts of Europe to be there woven.

Although this peculiar excitation of the fibres of wool and hair has been observed for many years past, its origin and nature were not understood, and were ascribed generally in the manufacturing districts to "the weather," as affecting the material. Long practice, however, taught manufacturers the lesson that placing the bobbins of yarn aside for some weeks or months in a damp cellar, at certain stages of the drawing or "roving" processes, gradually reduced the evil. Experiments were also made (and carried out to some extent in practice) in which the wool was steeped in various

chemical mixtures, in which metallic salts proved the more successful; the reason, though not comprehended at the time, being their greater comparative conductivity.

Different vegetable solutions have also been employed with this object, but I believe that in all these applications, while the remedy has been tedious, and only imperfect, the result has been more or less objectionable, owing to the mildew in some cases, and interference with dying in others.

Practically, the plan usually adopted has been to store the bobbins of wool at intermediate stages of manufacture in moist places. I have seen, for example, at the extensive works of Messrs. Foster at Queenbury near Bradford, a series of cellars arranged like a great system of wine vaults, with bobbins of woollen yarn piled on the shelves instead of bottles.

It will be seen that such a system necessitates a great delay in the process of manufacture, and that whatever the period of retention of the material may be at these intermittent stages, the capital necessary for carrying on the business is proportionally increased.

In the woollen manufacture millions of pounds sterling are thus locked up, entailing a corresponding loss of interest amounting to some hundreds of thousands a year.

Upon giving my attention to this difficulty, I saw that the remedies previously applied must prove ineffectual, and on enquiry I found this to be the case. The idea at once suggested itself to me that the only perfect method of rapidly neutralising the electricity in and about the wool would be by supplying a conductor of electricity to every filament of the excited material. To do this it occurred to me to place it in a closed metallic chamber, and then to exhaust the air, which, though a bad conductor under the ordinary pressure of the atmosphere (obtaining therefrom the general character of being an insulator), becomes, as we all know, a fairly good conductor when adequately rarefied. The chamber is connected with the earth by a metallic conductor.

The wool charged with electricity, not only on the outside, but throughout its closely-wound interior layers, is rapidly discharged through the rarefaction of the air extending into every fold. From 10 to 30 minutes suffice, according to the fineness of the yarn or

thread, and the closeness with which they are laid on the bobbin. In practice, the exhaust of the engine at the works has been used for producing the requisite degree of vacuum, but I consider a separate air-pump preferable.

The only appliances are the chamber already referred to connected to the exhaust of the engine, with cocks for admitting air or turning on the exhaust as required, and I recommend that they should be made of a convenient shape and size for wheeling in trucks laden with the bobbins.

A modification of this arrangement may also be applied to the carding or combing part of the machinery, by which a considerable saving in the wool breakage, and consequent increase of the long wool (technically called "tops"), would result.

By means of boxes arranged along the side of the factory rooms, the electricity generated can be withdrawn or neutralised at any stage of the manufacture, as in a few minutes the effect of the rarefaction permeates through every interstice, and affords, not only to the outside, but to the inside of every hair, relief from the electrical excitement to which it has been subjected during the friction of hair upon hair in the manufacture.

It might be assumed that, as the electricity generated is of a highly volatile character, it could be dissipated by connecting the spinning machinery with the earth; but this would only intensify the electrical development, inasmuch as the wool or hair consists of hollow tubes, as shown in the diagram, to which I now refer, of an enlarged hair. The friction induces an opposite electrical condition between the inside and outside of the wool tube, and the addition of an earth connection with the outside must tend to increase the electrical difference, as it does when a Leyden jar is being charged.

My system has been successfully introduced, and is about to be extended. I am satisfied that finer yarns and thread will be made by this means than has hitherto been found practicable, coupled with a large saving in labour, working expenses, and capital.

In the diagram, A is the section of an iron chamber which can be exhausted of air when the door at the end is shut, and a connection established with the air pump by opening the valve. B is an trolly filled with bobbins of electrified wool, C C C.

D shows the door of the chamber when open to admit the trolly.

On the door being shut, the valve, E, is turned to connect the air pump with the chamber for the requisite time, and then turned in the opposite direction to admit air again to the chamber by the pipe, G. F is a gauge indicating the degree of exhaustion.

Mr. W. H. PREECE: Mr. President,—I think it is only fair to Mr. Bright for me to say that in his own laboratory I have seen him perform these experiments (those which accompanied the paper) with absolute and perfect success. No doubt their partial failure was due to the apparatus having become to an extent moist from the natural effects of the occupation of this hall. I would say that some three or four years ago one of the wealthy merchants in the North was troubled very much with the peculiar phenomena referred to by Mr. E. Bright. Mr. Gilpin, the Superintendent of the Postal Telegraph Department of Bradford, was requested to visit the workshops (Yorkshire millionaires are not electricians), and he at once saw that the cause of the threads "standing on end, like quills upon the fretful porcupine," was statical electricity. Mr. Edward Bright, simultaneously and independently, was working at the subject, and he has succeeded in curing it in the way he has just shown to us, and I can vouch for the efficacy of the arrangement.

The PRESIDENT: It is an extremely interesting phenomenon, and an ingenious mode of cure, and one upon which further discussion would be interesting.

Mr. M. ROBERTS: I notice that the bobbins shown by Mr. E. Bright are made of wood. I should like to be informed whether, if metal bobbins were used, there would be any difference in effect?

Mr. E. BRIGHT: The bobbins are of wood. I do not think a change to metal bobbins would affect the production of the electricity, but it would slightly accelerate its dissipation. Looking at the very large stocks of bobbins held by the manufacturers, I am afraid it would be a difficult matter to persuade them to incur the cost of a change to metal bobbins, though no doubt the latter would be preferable in some respects. I will repeat the experiments at our next meeting, taking precaution to prevent moisture interfering, as appears to have happened just now.

Subsequently the experiments were successfully repeated.

The PRESIDENT: The phenomena referred to by Mr. Bright are by no means uncommon in manufacturing operations. One of my own earliest experiences in electrical matters was in a calico-printing works, where long woollen blankets placed under the piece of calico, as they came over the printing machine, became very highly charged with electricity. The blankets passed from the machines into hot chambers for drying the colours applied to the cloth, and I remember, when I was a boy, charging a Leyden jar by holding the knob against the blankets, and then getting the workmen to stand in a circle and have an electric shock. No doubt similar effects are produced in many manufactures, though they do not usually interfere with the process to the same extent as in the case that Mr. Bright has brought before us.

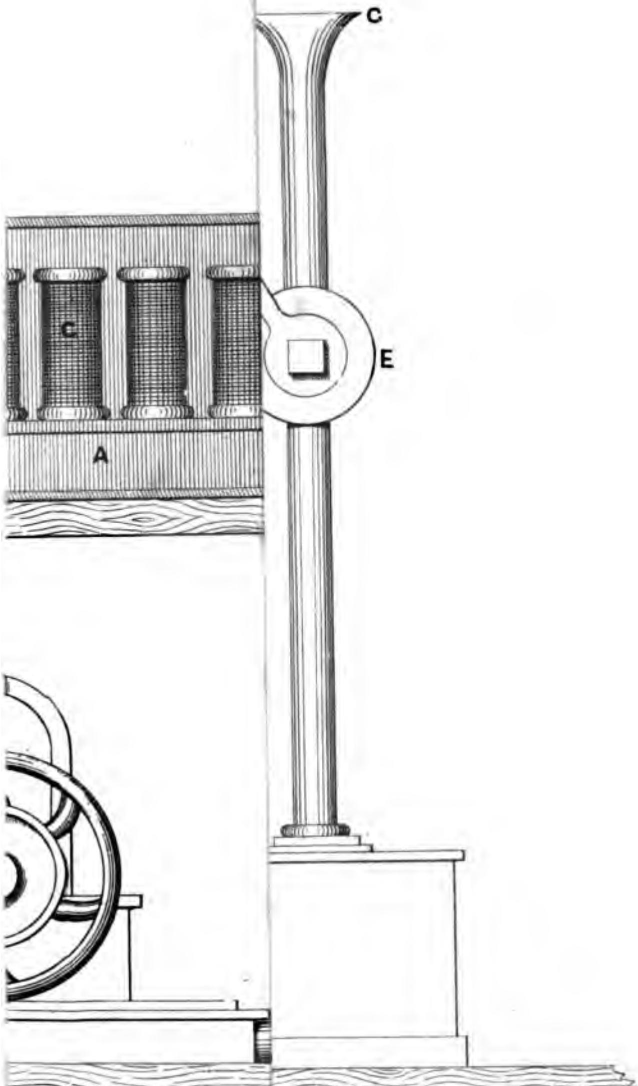
A hearty vote of thanks was passed to Mr. E. Bright for his paper.

A ballot for new candidates then took place, at which the following were elected, and the meeting adjourned until Thursday the 24th March next:—

Foreign Member:	*Members:*
Lieut.-Col. Hijinio Vallejos.	Lieut. Alfred C. MacDonnell, R.E.
	Charlton Wollaston.

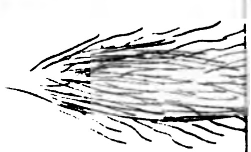
Associate:

Fielding Chevallier.



UMP.

UST CHAMBER.



"Stick

The Ninety-eighth Ordinary General Meeting of the Society was held on Thursday evening, March 24th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. FOSTER, F.R.S., President, in the Chair.

The SECRETARY read the minutes of the last meeting, which were put and confirmed.

The PRESIDENT: I have to announce that Professor Helmholtz is coming to London shortly, to deliver the Faraday lecture to the Chemical Society. An intimation has reached the Council of this Society that probably Professor Helmholtz might set apart one evening of his stay in London to meet the members of this Society. Should such a probability be realised, the evening which has been mentioned as likely to be fixed for the purpose is Monday, April 11th. The Council have appointed a Committee to consider arrangements for the suitable reception of the Professor, and information of whatever may be decided upon will be given to members as soon as possible. (Applause.)

The names of new candidates were then announced, and their nomination papers suspended.

The following paper was then read:—

TELEGRAPHS IN JAPAN.

By J. MORRIS, Member, *Local Honorary Secretary for Japan.*

Nine years ago Japan was described as a land of which little was known, and still less understood. Western civilisation has since that time progressed rapidly, and at this day it must be admitted that an average acquaintance with the general characteristics of the country and its people is now possessed by the reading public of the world.

Railways have been constructed and worked on a small scale between the present capital of Tokio and the open port of Yokohama, and between the ancient capital of Kioto and the ports of Ôsaka and Kôbè—in all, about 70 miles; and the benefits accruing

to the possessors of rapid and effective means of transit are so far appreciated by all classes that very considerable traffic is now being conducted thereby, and the Government is vigorously extending the system to places further inland—the centres of trade in tea and silk.

Mining operations in iron, gold, and silver are receiving a fair share of attention, and with the prospect of ultimate success—though the scarcity of good roads, apart from the celebrated one which traverses the country from north to south, is likely to prove a serious obstacle for some years to come.

Lighthouses have been erected around the coast to an extent which renders the otherwise dangerous seas of the Japan Islands comparatively easy of navigation, and have so far been maintained with a noble disregard of the "light dues" enforced in some countries upon shipping.

The advantages derived from the existence of a powerful fleet of merchant steamers were unmistakably felt in connection with the suppression of the late insurrection in the island of Kiushiu; but the fact must not be lost sight of, that even this powerful weapon could only have been inefficiently wielded had it not been that the system of land telegraphs extended to most of the important towns of the empire, whereby the Government was not only placed almost instantaneously in possession of details of the outbreak, but was subsequently enabled to make such disposition of its military and naval forces as to effectually quell a rising which at one time seemed to threaten to end in another revolution.

It is, of course, with the past history and present doings of this branch of the public works service that I venture to trouble the members of the Society to-night; and, while entering upon the task with great diffidence, I trust I may succeed in placing some few facts before the meeting which may not be wholly uninteresting.

As far back as the year 1869, attention had been directed to the telegraph, and two short lines had been provided and worked by Bréguet alphabetical instruments, from Tokio to Yokohama, and Ôsaka to Kôbè—in all, about 40 English miles; but it was in 1871 that a general telegraphic system for the empire was decided upon. Engineers were then engaged from England, and upon

their arrival in the autumn of that year a rapid preliminary survey of the country was undertaken, and arrangements made for the provision of the requisite timber—the fittings in the shape of insulators, brackets, etc., as, indeed, wire and apparatus generally, having been ordered from England. In the course of the next spring and summer, communication was fairly established through one-half of the principal trunk line, and in the ensuing autumn the entire length of close upon 900 miles was completed and opened provisionally for traffic, though as yet without formal official sanction, that being reserved until the elaborate system of lines which had been agreed upon for the whole country could be carried out in its entirety.

Innumerable little difficulties, as may be imagined, were met with in these early days. The people of the interior had scarcely become reconciled to the new order of things consequent upon the revolution of 1868; the old feudal system was practically in force, though nominally abolished; the roads were but very imperfectly known even to the native staff; superstition and dislike to the introduction of Western notions, with general hatred of the foreign "barbarian:" all had a share in rendering the establishment of the telegraph in the interior anything but an easy task, or absolutely devoid of personal danger. Happily, this has long ceased, save in times of unusual excitement among the peasantry, and such instances are very rare.

The timber employed in these earlier works was not carefully selected,—a great deal was sacrificed to speed of construction,—yet, nevertheless, some of the poles then set up are in existence to this day. Most of them perished, however, with the third or fourth year of their existence, and all were replaced, with very few exceptions, within six years.

The tree from which the poles are commonly cut in Japan is the "sugi," a species of cedar (*Cryptomeria Japonica*); other woods are employed to some extent, among them the "hi-no-ki" (*Retinispora obtusa*), and this last appears to be less affected by dry-rot than the cedar. Owing to various causes, however, there exists a great difficulty in the way of obtaining properly felled and seasoned timber; and it is unlikely that this will be diminished

from the fact that the supplies of wood in the immediate neighbourhood of the main trunk lines have become practically exhausted, and we have to look to the more remote inland hill-sides to yield sufficient for the annual maintenance requirements.

A few iron poles were imported, but they were too short in length to be generally employed, even if the first cost and subsequent charges attendant upon their transport from place to place had not been so vastly in excess of the price paid for wooden ones, which averages about 3s. 6d. for 24-feet lengths, gradually increasing to the 50-feet lengths, for which, however, a very disproportionately high rate is demanded.

The life of "sugi" poles naturally varies in duration very much, according to the soil upon which the tree has grown, and to that in which it has subsequently been erected; as a rule it is not safe to expect that it will endure beyond four years, and decay, in its earliest stages, frequently makes its appearance within six months. Tarring has been extensively resorted to, and in some intances with success; from the trouble and expense attending transport of tar to the country, however, it has latterly been found unadvisable to persevere, and the cheapest and simplest course appears to be to replace the poles after a four years' term, or, at least, as soon as decay has progressed so far as to make renewal desirable. This may sound somewhat strange; but timber and labour are so cheap in the provinces, compared with the cost of transit of preserved timber by water or other means, combined with the fact that a numerous staff of linemen is everywhere regularly employed and available for repair work, it is a question whether a better policy could be pursued, at least at present. The Boucherising process has lately been tried, and with a fair measure of success, as far as the first cost of poles so treated is concerned; but the calculations are based upon the assumption that this timber will be not less than three times as durable as that which has not been subjected to similar treatment: this the future can alone decide, in regard to Japanese wood. Creosoting is quite out of the question, owing to the enormous expense of sending the poles to and from the works, if these were established at the central depot, or even if at several convenient points in the interior, everything having to be con-

veyed on hand-barrows or on men's shoulders for many miles. So much for the item of timber.

The short lines previously referred to as having existed prior to 1871, were worked by Bréguet instruments, but the new and long lines thereafter provided have all been worked by Morse apparatus supplied by Siemens Brothers, of the single-current pattern, and these have given thorough satisfaction. Upon the longest line, working between Yokohama and Nagasaki, automatic translators are employed at Kôbè, but occasionally in very fine weather the wire has been worked direct without their aid, the batteries being of the value of about 80 cell Daniell, and the relays of 900 ohms resistance.

The Daniell battery is ordinarily employed throughout the system (a few Leclanché cells are in use for call-bells merely), and a stock of plates, cells, and copper sulphate is supplied to every station in advance, sufficient for six months' consumption.

The Bréguet instruments were retained in use upon the short local lines in the capital for a considerable period, but gradually retired in favour of apparatus of the Morse type. Single-needle instruments were employed upon the Railway Department's wires between Tokio and Yohohama in 1872, and subsequently on those of the Kôbè and Ôsaka section, opened two years later; but these also have been replaced by the Morse system, with the object of attaining uniformity in regard to the apparatus in use as far as practicable. The "single-needle blocks" originally used on the railway are still, however, in operation for train-signalling.

At first the wire was everywhere suspended from insulators of the Varley double-cup earthenware pattern, fixed in Warden's brackets, but it was shortly found advantageous to make use of wooden arms, particularly as there exists in the country a very suitable wood, known as "keyaki," and also a species of oak, both everywhere obtainable at moderate price. Moreover, it was natural that, in a country famous for its porcelain, attention should early be directed to the production of a material suitable for insulators. The first attempts in this direction were comparatively failures, and the home-made article turned out anything but well; but want of success at the outset did not cause the manufacturers to

despond, and in the end a porcelain single-cup insulator was produced, which at once was found to answer all requirements, and has since given general satisfaction, dating from the year 1874.

The best material for porcelain to be found, for the purpose of making insulators, was obtained from a hill in the island of Kiushiu, regarding the use of which there was formerly the strictest prohibition, save when the articles to be made were destined to grace the palace of the Tycoon ("Shô-gun"). Insulators of this class, when tested, were found to be of a quality beyond the cacapity of a Thomson reflecting galvanometer of 20,000 ohms resistance, unshunted, with a battery of 200 Daniell cells, to determine.

(Several are sent herewith for inspection, as well as a double-cup insulator made as an experiment, but too costly for general use.)

At the present time an instrument of the same resistance, etc., is employed to test every insulator before it leaves the works, the standard being fixed at 40,000 megohms. Those made of the famous "Imari" or "Hizen" ware only fail to the extent of ·797 per cent. to pass the ordeal; others of Kioto ware are not quite so good. Defective cups are destroyed immediately. Later on I may be permitted to offer some statistics relative to the actual tests taken daily for insulation.

In connection with the establishment of the line joining the capital with Nagasaki in 1872, it became necessary to lay a submarine cable across the Straits of Shimo-no-seki, the western entrance of the "Inland Sea," which divides the island of Kiushiu from the mainland of Nihon. The Straits vary in width from two miles to only one-third of a mile at the narrowest point, and the most suitable position for the cable was found to be a little to the eastward of the narrow channel, where the width is 1,350 yards. The requisite length of shore-end cable was taken down from Yokohama by the steamer attached to the department, and the submersion was readily effected by means of small native lighters towed by ropes carried ashore, and there hauled in by some hundred or so of the villagers and fishermen. This cable has remained in perfect condition to this hour. Four others have since been carried across at the same point.

The only difficulty attending cable-laying operations in these

Straits is occasioned by the very strong tidal currents, which sweep through the narrow channel with a velocity frequently exceeding 8 knots per hour: the period of slack water, too, is of so brief duration as to be almost undistinguishable. The cables are terminated upon Siemens' pattern plate dischargers, in convenient huts built for the purpose.

The experiment was tried of suspending an open wire across the Straits from poles erected on the adjoining hills, the length of span being about three-fourths of a mile. Owing to the lack of really suitable wire, it was not altogether successful, and after hanging for a week the wire was caught by the mast of a passing man-of-war.

There is no special difficulty to be contended with in the provision of an open line at this spot; but as the submarine connection is absolutely safe, and costs practically nothing for maintenance (if we leave out the item of very gradual deterioration), it is unlikely that the experiment will be repeated.

At Imagiri Inlet, about midway between Yokohama and Kôbè, it became necessary to carry over the line on poles set below water level, in some cases to a depth of 12 feet, the width of the inlet being about 2½ miles. This proved a somewhat tedious and expensive operation, especially as sea-going native vessels pass out by the central channel, the masts of which are so long as to necessitate the wires being kept 60 feet clear at high water. The works were substantially executed, and the line existed, with occasional mishaps, until the poles decayed in 1877 to an extent which rendered renewal imperative; and advantage was then taken of a by-road passing around the head of the inlet to do away with the crossing at its mouth, by the substitution of an entirely land line, which, though involving extra mileage, is less costly to maintain, and is practically secure in stormy weather.

The rivers of this country have proved a source of considerable trouble, especially in the hot season. In winter many are all but dry, but in floods during the summer and autumn months the channel becomes a mile or so in width in two or three of the most important rivers of the east coast; and the velocity of the current at such times is prodigious, quantities of timber, even houses and

cattle, being brought down from the hills and carried on with irresistible force to the open sea. The width from bank to bank being so great, it is not practicable to span such rivers, and consequently poles have to be planted in those portions of the flood-channel least likely to be disturbed by the scour. The earlier attempts in this direction were failures, but ultimately a system of construction was adopted which has for five years proved efficient to withstand the rush of the heaviest floods, and there is now no danger of the long-continued interruptions to communication which frequently occurred prior to 1874, when poles were regularly washed away in spite of all attempts at affording protection by fenders and embankments.

The widest of the east coast rivers, the Ten-riu, is now crossed in four spans averaging 450 yards each, the double ("H") masts being 60 feet high, and the wire employed of No. 11 gauge. The river Oigawa, where the floods are most serious, is crossed in three spans of about the same length, at a point where the banks approach each other, somewhat higher up stream than the regular ferry. All the poles are well protected by fenders, in a V shape, formed by piles driven in to a depth of 12 to 16 feet, the wire stays, in the same way, being attached to piles, so that they can readily be tightened if ever necessary.

Many attempts have been made to bridge these rivers, and no doubt immense benefit would accrue to the traffic on the highroad if this could be effected; but, so far, the wooden structures provided in the winter months have annually been swept away by the first floods, and probably nothing short of iron screw-piles, carried down to a great depth, would afford any security as bridge supports, while, the beds being composed of large stones loosely mixed with sand far below the surface, boring operations are likely to be considerably impeded.

I was told that in the Fujikawa, another of these troublesome streams, the borings showed sand to a depth of 90 feet.

The frequent changes of channel rendered the preservation of a line of poles peculiarly hazardous, and it became necessary for this reason to reduce the number of supports to the lowest possible, and to make as long spans as consistent with safety in the much-dreaded

typhoons which frequently blow with hurricane force around these islands in the autumn.

At the outset but one wire was suspended from the poles on the Southern trunk line. It soon became evident that the telegraph would be a popular institution, and greater facilities were needed for the rapidly increasing traffic: a second wire was therefore commenced in the same year. Extra wires have since been carried through, and at the present time there are 5 wires connecting the capital with the South, irrespective of the numerous branches and local lines, and of two alternative routes by the middle and west coast roads.

The total mileage on the 31st December, 1879, was as under:—

Poles	3,929¾	English miles.
Wires	9,345	ditto.

In 1871 a school was established for the training of the Japanese youths as operators.* They have year by year been educated and sent out to the various stations as rendered necessary by the extension of the service. In the year ending June 30th, 1880, in all 227 were appointed to new offices or sent out as reliefs: 97 remained under tuition.

These scholars are taught to write and speak English, and also French to some extent, with the rudiments of an English education generally; and they have to qualify as Morse operators by sending and receiving messages at a fair rate of speed previously determined upon.

The Japanese language having no regular alphabet, it became necessary to form a combination of Morse characters to represent the sounds of the syllabary known as the "Katakana." This was effected by using the letters of the International Code, supplemented by others formed of *five* dots and dashes (figures, of course, excepted), to produce a total of 47 signs, and the "native alphabet" so constituted has given tolerable satisfaction during the nine years which have passed since its introduction.

* This is quite distinct from the more advanced courses of telegraphy which have been given since 1873 at the Imperial College of Engineering, Tokio, to those of its students who select the telegraph as their profession.

The maintenance of the lines throughout is performed by a staff of inspectors and linemen, who have all undergone practical training on new and repair works; the inspectors being required to qualify themselves for the general indoor duties at offices, such as the localisation of faults in wires or apparatus, battery testing, &c., before receiving their appointments. The linemen have nearly all been taught (under the eye of a European employé, at some time or other) the methods of fitting and setting poles, jointing of wires, etc., and they can be trusted, as a rule, to execute minor repairs with reasonable despatch and skill.

It is a rare occurrence now to find an interruption lasting more than half-a-day on the trunk lines, unless in some exceptionally bad weather, or, as happens in the North in winter, accumulations of snow make the roads impassable.

The workmen thus employed are divided into five classes, and distinguished by stripes on the blue cotton livery supplied to them twice-a-year: on the back they bear the badge of the letter "Den," or "lightning."

In some few instances such men have risen to the grade of petty officers, but, as considerable acquaintance with Chinese writing and arithmetic is essential, the number of those who have so distinguished themselves is very limited.

In the ten months of the present year ending October 31st, there were 153 interruptions of less than 6 hours' duration, 85 of less than 12 hours, 27 less than 24 hours. 21 other faults exceeded this limit, but only 7 of them were of a serious character, and were due to floods in the remote provinces, which stopped all traffic by road.

If we have regard to the total (87) number of distinct *circuits* for the 304 days, we find an average of somewhat less than one interruption per day on 9,345 miles of wire.

The system originally provided one man to about every 15 miles of line—the man living in the town or village midway through his section, and walking half of the entire length each day (Sundays excepted). For economical reasons, and to afford more complete control, however, the men now reside at the stations (which are only 30 miles apart on an average along the

trunk line), excepting in some special cases, and go out whenever a fault appears, late or early, patrolling their sections only once a week or so, and this plan seems to answer every purpose.

The extension of the telegraph has produced some interesting results in regard to the rice trade. In former years a stock of the staff of life equal to 1½ years' consumption was always kept on hand in the granaries of the various castle towns, the residences of the numerous feudal lords; but with all precautions famines would occur in the more remote principalities. This, happily, is now impossible, owing to the readiness with which supplies can be concentrated upon any district threatened with scarcity by failure of the crop; and thus such large reserves are no longer needed, and quantities of grain can be placed upon the market which otherwise would be lying idle, and certainly not improving in quality.

Telegrams are constantly exchanged between Japan and the principal centres of the tea and silk commerce in Europe and America; and, upon the receipt of information from abroad in any degree affecting the exports from Japan, it is immediately distributed to the agents of all the principal merchants in all parts of the empire. In regard to these products, therefore, the telegraphs may be said to have practically worked a revolution in the method of carrying on mercantile operations. The value of all products, agricultural or otherwise, has vastly increased since the establishment of telegraphic communication, owing to the greater facilities afforded thereby to trade generally, and this may be said of property of all kinds. Some idea of the extent to which the "wonder-working" wire is employed by Japanese merchants may be gained from the fact that recently, when the Government had to prohibit speculation in rice and paper currency for a brief period, the receipts at one office alone fell off to the amount of £70 as compared with the day before. Before telegraphs existed, relays of messengers, fleet of foot, were kept constantly in readiness, night and day, to convey despatches affecting the prices and shipments from the various ports—men who maintained a speed of 9 miles an hour for a distance equal (in one case which came directly under my own observation) to 65 English miles. Across the Straits of

Shimonoseki, where five submarine cables now exist, the signalling was done by hand flags and lanterns.

The total receipts for the year ending June 30th, 1879, from all sources, amounted to £108,323; and the total expenditure (if we except the item of cost of building new lines) to £101,674, so that, for the the first time in the history of the department, its revenue exceeded its working expenses. The new extension works involved a further outlay of £25,809.

The total number of telegrams dealt with during the year was 1,272,756, of which about 96 per cent. were in Japanese: international messages numbered 22,695.

The proportion of telegrams in the native language averages about one to every 30 individuals, taking the population at 35¾ millions, as determined by the latest census.

The rate of increase in traffic and receipts is represented roughly by the diagrams which I have the honour to submit for the inspection of the members of the Society.

The experience of the last few years has shown indisputably that the tariff for native messages was originally framed on a basis too low to make the Telegraph Bureau a highly paying institution, but a low rate was adopted with the object of inducing the public to make general use of the system, and in the fear that a high tariff might have a prejudical effect. Doubtless, at this moment, so much dependence is placed upon the means of rapid interchange of news between the chief cities and ports, and the wires play so important a part in mercantile life, that an increase in charges would but little affect the traffic in bulk; but after the lapse of 9 years it is not thought advisable to make any change in this direction.

The average rate for 20 characters of the Japanese language, for a distance of about 60 miles, is roughly 3 sen, or, at present rates, less than one penny, taking the entire line from Tokio to Nagasaki as a basis of calculation. Of course, the average for a shorter distance is higher; but the messages between Tokio and Yokohama are transmitted for 7 sen, which is about equal to 2¼d. for 20 miles.

The rates established for the foreign traffic throughout the

country have been based on the fact that it costs more for their transmission,—skilled clerks are specially needed,—and as high a rate of speed as compared with Japanese traffic cannot be maintained. The average price for 20 words (exclusive of the special international rates) amounts to about one-tenth of a penny per mile, for messages passing between the capital and Nagasaki. In the last fiscal year, the income derived from foreign telegrams in this way was £4,719.

Submarine cables exist at places around the coast other than Shimonoseki: there are two in the "Inland Sea," connecting Nihon with Shikoku, at a point where the channel is 6 miles wide; and two across the Tsugaru Straits, near Hakodate. The lighthouse steam tender "Meiji Maru" (a Clyde-built vessel) was employed in connection with the submersion of the Shikoku cables, and the Great Northern Telegraph Company's "H. C. Oersted" was engaged to lay those at Hakodate.

The Shikoku cables originally consisted of "deep sea Atlantic," with corresponding shore ends, but were never very strong, owing to previous deterioration during the 6 years the material lay at Yokohama without (at first) proper accommodation. The fishermen, moreover, frequently hauled them to the surface, unintentionally perhaps, when getting up their anchors, and then in ignorance of their electrical value, set themselves free by the primitive method of dividing the wires with a hatchet; finally, the *teredo navalis* completed the work of destruction by boring innumerable holes in both.

It was not until January of this year, when a strongly protected cable, 2¼ inches in diameter, with 2 conductors, was laid, that confidence in our ability to maintain a reliable line of communication at this point was fully restored.

In the North several mishaps have occurred, and at present only one of the two cables is at work: it is intended, however, during the ensuing spring, to lay a new one, with two conductors, from Imabetsu Bay direct to Hakodate, thus avoiding a long land line liable in winter, at any moment to be stopped by snow. The length of this cable will be about 33 miles.

Tests for insulation, and on fine days for conductivity twice-a-

week, are regularly made every morning at 7 o'clock, at three important stations, viz., Tokio, Kôbè, and Nagasaki. As all the principal lines radiate from these three centres, accurate knowledge is possessed at headquarters of their condition by about 9.0 a.m., including some idea of the state of the weather everywhere.

Insulation is determined by a tangent galvanometer of 41 ohms resistance—conductivity, by the Wheatstone bridge. The wires in wet weather rarely give a lower insulation resistance than 1¾ megohms per mile, while in fine weathor, the Southern lines, for example, show no visible leakage when tested to Toyohashi, a distance of 180 miles, which implies that the resistance is greater than 90 megohms per mile.

Much has yet to be done in the way of obtaining accurate meteorological reports by telegraph from the many distant stations. Great benefits must follow from observations of wind and weather on these coasts, peculiarly fitted as the islands are from their geographical position for such scientific investigations.

The absence everywhere at present of iron-works, etc., in proximity to the lines doubtless is a great advantage, as far as insulation is concerned; but, on the other hand, the highroads are bordered by *Cryptomeria*, and in summer the branches of these are the resorts of countless spiders, which endeavour, and not without success, to counteract the beneficial effects of fine weather on the working of the lines by spinning myriad threads of gossamer between the earth-wires, wooden arms, and insulators and the trees which they infest; and nothing is needed beyond the heavy night-dews to render these fairy conductors almost perfection in their capabilities of working disaster. Men are constantly employed in sweeping the wires with bamboo brushes at this season, but the difficulty can in no way be completely overcome.

In the course of the year 1877, during the struggle with the Satsuma rebels, recourse was had to temporary lines of telegraph to a great extent, and were instrumental in bringing the war to a speedy termination, by reason of the facilities which they gave to the commanders of the loyal army for concentrating their forces quickly at given points, and for counteracting the effects of the

rebel leaders' tactics in the field. The Satsuma men made forced marches, and might have effected many surprises, but that the telegraph was ever at hand to defeat their schemes, and to give opportunity for employment, to the best purpose, of the Government troops. No fewer than 511 miles of line were construsted with this object, and 53 offices were opened at villages immediately in the rear of, or in direct connection with the afterwards victorious army. The materials employed in construction were light portable posts, vulcanite insulators, and No. 11 wire, every use being made of natural supports in the way of trees, etc., when practicable; and for insulators, when the stock became low, any thing in the shape of earthenware that could be picked up. There were but few interruptions to working of a serious character.

On the 31st December, 1879, there were 112 offices open for general traffic, local and international, and 70 others connected with Government departments, railways, or police. 53 in all are kept open day and night. There are 348 Morse instruments in use, 25 single-needle blocks, and 29 telephones of the Bell pattern. Some of the latter have been made in the workshops, and answer very well. A pair of Edison telephones have been tried in Tokio privately, with excellent results.

Seventy-one instruments of various kinds are fixed in the school for the students to practise upon.

The staff of the department on the 31st December, 1879, numbered 1,803 individuals of all ranks, of which 496 were inspectors, linemen, and workmen, 707 were cashiers and clerks at stations, and 358 messengers—the remainder being engaged at headquarters as correspondents, writers, etc., or at the depot as accountants, mechanicians, etc., and in various other capacities.

The European employés now number 10 only, many having returned home on the expiration of their engagements, having been released by the Government in the belief that many of the duties of supervision and instruction can now be performed satisfactorily by the Japanese staff.

In conclusion, I beg to present a few photographs taken from negatives of my own, which may serve in some way to indicate the character of the construction works carried out in Japan in

connection with the rivers, etc.; and further, to submit a paper containing some statistical information furnished by the Japanese electrician relative to the tests of wire and insulators, particularly with regard to the results obtained from copper wire of native manufacture, insulated with *lacquer*, as a substitute for silk or cotton, and which promises to become a material of great service.

A special list of the wires, taken on the 1st and 2nd instant, for insulation in fine weather, with the object of ascertaining the exact resistance, gave as the total rate per mile, on a line 183 miles in length, the high record of 314·9 megohms.

TOKIO, *December*, 1880.

The PRESIDENT: The Society will, I am sure, recognise the great interest of this paper, which gives us another proof of the energy and intelligence of the Japanese Government in introducing beneficial improvements into their country.

There are many members present who have had experience in somewhat analogous work in countries other than Japan, and the Society will be glad to hear any remarks they may have to offer on the paper.

Mr. J. A. BETTS: I have had no experience with telegraphs in Japan; and in China, where I was torpedo engineer to the Government, there is not at present a very extensive telegraph system. I believe that the first telegraph put up in China for the Chinese Government was constructed by myself. In my official capacity I had to instruct some native students in working the Morse instrument, and found them apt and quick. I therefore thought it best to construct a short line of 6 miles, and while that was being done the students practised on instruments in short circuit, and became proficient for proper working by single-current direct Morse when the line was completed in May, 1876. The stores for the line were obtained from England, but the construction was carried out under my supervision, without foreign assistance. The line was erected across a plain which in the ensuing winter became flooded with water. Frost and storms succeeded, and the line was carried away on the ice; but although the wires were much stretched, and touching the ice (which was very dry) in many places, working

was not actually stopped. The poles were refixed in the ice, in which holes had been dug with ice-picks, and the line remained all right until its removal to another route in the spring. Mr. Morris has referred to the opposition shown by the natives to the construction of telegraphs in Japan. Such was not my experience in China, where I had less trouble of that description than I have experienced in England. In some cases in China opposition was made through fear of the "evil spirits" coming into their houses. The line I have spoken of was erected for military purposes, and was not intended to be used for public messages; in fact, the Viceroy, Li Hung Chang, objected, on the ground that the line was solely for Government purposes. Afterwards he was persuaded, on financial grounds, to relax his objection, and in June, 1879, the line was extended from Tientsin to Taku Forts, a distance of 40 miles, and opened for traffic at the rate of a dollar a message of 10 words. In the first month the line paid more than its working cost. A code-book was adopted for the transmission of Chinese messages. A Frenchman had compiled a code-book with some 5,000 or 6,000 characters of the Chinese language, and put a number against each. By sending these numbers, and at the receiving end obtaining explanation of them by reference to the code-book, the transmission was completed. The arrangement answered very well, and continues to do so, as I have lately heard from Tientsin. The foreign use of this telegraph is almost exclusively for signalling vessels riding outside the Taku bar when there is not sufficient water to cross. No doubt China is more conservative in regard to this matter than Japan, but there is perhaps one of the finest fields for telegraphs and railways in China that have ever been opened out in the world, and it is just about to be opened. The Great Northern Telegraph Company are commencing to put up a line from Shanghai, and are under contract to put up about 4 miles a day for a line of some 700 miles, going straight away to the capital of China. The Chinese Minister in London informed me yesterday that it was under the serious contemplation of his Government to open up railways in China to Pekin.

The PRESIDENT: Mr. Morris has sent an appendix to his paper, giving an account of the specimens which he has sent.

The SECRETARY then read the following

MEMORANDA RELATIVE TO THE SPECIMENS SENT.

Lacquered Paper Chamber.—This chamber has been invented by H. Oki, manufacturer of telegraph instruments in Tokio. It is made of Japanese lacquer and paper. Every sheet of paper is cemented with specially prepared lacquer, and finally linen is covered over it, and lacquered again. This chamber is strong, although not very high in insulation, and will stand rough treatment: in our military telegraphs, therefore, all the battery chambers are of this kind.

Lacquered Copper Wire.—This process of covering wire has also been invented by H. Oki, whose chief object is to get a covered wire very cheap and efficient. Coils in his instruments are wound with it, and every layer of wire coated with paraffin to insure complete insulation. Instruments so made have been working for about a year. The enclosed coils are samples of the wire. It is common commercial copper wire, tested before covering: its specific conductivity is 90·7 per cent., and insulation between the layers of the wire practically perfect. A length of 10 feet was carefully suspended from insulators, and its resistance ascertained to be 2·67 ohms at 52° F. The coil is said to consist of 111 feet, and should therefore give a total resistance of 29·637 ohms: it actually gives 29·58 ohms, showing a difference of ·057 of an ohm merely, which is doubtless due to slight variation in diameter. Larger coil not tested.

Silk-covered Copper Wire.—This wire was first made by S. Miyoshi, a mechanician in our workshop. Both the wire and silk will gradually be improved.

Morse Ink and Carbonic Paper.—These two substances are made by T. Taoka, a mechanician in the workshop. Both are now exclusively used in the offices of the empire.

Insulators (4).—These are made by Fŭkāgāwā, porcelain manufacturer at Arītă, in Hīzen, near Nagasaki. Their resistance is marked on the labels.

Porcelain Chamber and Porous Pot (circular).—These are also manufactured by Fŭkāgāwā, and are used in the insulator

testing battery. The other oblong-shaped porous pot is in general use, together with the porcelain chamber, made at Seto, in Owārī, near Năngoyā.

Morse Paper.—This is made by Sēishī-shā, at Ojī, near Tokiyo, and is now exclusively used in the offices of the empire.

I have not been able to prepare all the specimens I should like to send, but I hope to forward a more complete collection on another occasion.

M. YOSHIDA.

TŌKIYŌ, *December 28th*, 1880.

Mr. DRESING said, in reply to the President's call: I have not been in Japan, and cannot, therefore, give you any information about the Japanese telegraphs.

The Great Northern Company have got the submarine cable communications to Japan, and have, as Mr. Morris has pointed out, assisted the Government in laying some of their submarine cables: this is, as far as I am aware, the only active part the Great Northern Company have taken in the construction of the Japanese Government telegraphs.

Mr. JOSEPHS: I am not acquainted with Japan, but in India and other foreign countries I have always met with difficulties in the way of carriage and provision of material, similar to those described by the gentleman who read the paper.

Mr. Josephs being invited to relate his experiences when carrying wires in Northern India for the Afghan expedition, said he was not prepared at the moment to do so, though at some future time he hoped it would be possible to bring the matter before the Society in the shape of a paper.

The PRESIDENT drew the attention of members to the specimen of materials used in Japan, which were on exhibition in the room.

Mr. W. H. PREECE: I have not yet had the opportunity of closely examining the specimens exhibited, but from a first glance they look more like articles that come from Lambeth or Stoke-upon-Trent than from Japan, and the porous and earthenware pots seems to be of better stuff than we usually employ in England. What has surprised me most, and looks most promising, is the lacquered wire. There seems to be something to be gained by

using this wire in making electro-magnets. The closer the layers of wires, and the greater the number of convolutions round a bar of iron in a given space, the greater will be the strength obtained in the magnet; and any invention which will remove the necessity for the use of silk or very fine cotton, or cotton and silk mixed, without increasing the space, will certainly be beneficial to telegraphy generally. I have been much interested in the paper, not because I have ever been to Japan, but because I look upon a paper of this kind as one of those very things for which this Society was specially constituted. It was specially constituted to bring together in one central point (and that point the centre of the world at the present moment) experiences, ideas, and investigations from all parts of the globe. We have had papers from Africa, Persia, and India; we now have a paper from Japan; and we have the promise of a paper describing telegraphic operations in the north of India in connection with the military expedition to Afghanistan. That part of Mr. Morris's paper which refers to the wonderful power conferred by electricity of taking steps to ameliorate the sufferings caused by those fearful catastrophes (which seem part and parcel of one's existence in Eastern countries) is very striking. In India, where British rule has been extant for some 100 years, where the enterprise and power of the empire has been devoted to the extension of railway and telegraph interests, we have not succeeded in entirely eradicating famines; but we find in Japan that one of the greatest blessings that the great power electricity has given us has been showered on that nation by reducing the tendency to suffering from famine.

The cheapness of transmission of telegrams in Japan, too, is remarkable. Where else in the universe besides Japan is it possible to send a message 20 miles for 2½d.? The presence of spiders (and such spiders) referred to by Mr. Morris must be very troublesome, and every practical telegraphist present will feel glad that he does not live in Japan where his wires would be coated with such masses of gossamer. We have spiders in England, but they are poor, miserable, weak creatures compared with those in Japan, which get their web across the wires and put them to earth.

The ingenuity of the Japanese in making the various articles

exhibited deserves notice. They have not only produced by native manufacture, insulators, porous cells, and ink bottles, but they have devoted their skill to making telephones. Now, with a country devoid of patent laws, and with people possessing the ability of imitating, even to the trade mark, apparatus of different kinds, I would ask how is it possible for companies thrown upon our commercial market, with enormous purchase-money for patent rights and promoters' fees, to make a business in a country like Japan, where telephones can be constructed cheaper than in England, and perhaps as good, if not better?

I have pleasure in proposing that the best thanks of the Society be given to Mr. Morris for his paper, which must have cost him much time and trouble.

The PRESIDENT: In putting the vote of thanks to the meeting, I would ask Mr. Preece to allow me to include the name of M. Yoshida, the Japanese electrician, who has sent a description of the specimens exhibited.

A hearty vote of thanks was accorded to Mr. Morris for his paper, and also to Mr. Yoshida for his description of specimens.

The meeting then adjourned until Thursday, April 14th, 1881.

The Ninety-ninth Ordinary General Meeting of the Society was held on Thursday evening, April 14th, 1881, at the Institute of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. FOSTER, F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

The names of new candidates for election were announced.

The PRESIDENT then called upon Mr. St. George Lane Fox for his paper, which was practically illustrated by the hall being lighted by Mr. Fox Pitt's incandescent electric lamps.

ON THE APPLICATION OF ELECTRICITY TO LIGHTING AND HEATING, AND FOR DOMESTIC AND OTHER PURPOSES.

By ST. GEORGE LANE FOX PITT, Member.

The subject which I am about to enter upon this evening has been the cause of considerable difference of opinion among practical minds, and, I am sorry to say, also of considerable antagonism. Be that as it may, however, I think I am justified in saying that it may now be regarded as a certainty that electricity will be extensively used in the immediate future for general domestic purposes, especially those of lighting and heating. Having this fact in view, I think it would be highly desirable that electricity should be divested, as far as possible, of its mysteries, and the subject put before the public in as simple a form as possible.

For the last few years I have been experimenting on the more simple applications of electricity, and in my own mind I formed a conception of electricity which I have found to be of great service to me. It may not perhaps be out of place if I attempt to explain these ideas briefly. I may mention that possibly this theory is not by any means new, but, inasmuch as I have not seen it stated elsewhere, I assume that it is not, at all events, generally accepted.

Electricity itself I have conceived to be a subtle fluid of indefinite elasticity—that is to say, I conceive it as an extraordinarily attenuated form of matter, capable of diffusing and expanding itself in certain bodies to an indefinite extent—a fluid capable of being

compressed by the application of force, and again capable of expanding when the force is removed. This electrical force has been called variously "tension," "electro-motive force," "density," and "potential;" but I prefer, in order to simplify the matter, to use the expression "electric pressure." This fluid, when dealt with in practice, I always regard as existing under a certain amount of pressure, and as exercising an amount of pressure, in every direction, in the bodies which hold it.

But, although it is necessary to deal to some extent with the properties of electricity, in order to arrive at certain practical results, I prefer to deal with it generally as an important medium for the transmission of force or power. You are of course aware that an electric current does not necessarily mean an expenditure of energy. It is the force required to maintain the current through a resistance which represents the expenditure of energy; thus, to maintain a current, however great, through a conductor having no resistance, requires no energy whatever. I say all this because I am desirous to put the whole subject on a basis as clear as possible, and to show, if I am able, that theories founded on a vague conception of electricity cannot, for practical results, always be relied upon, but may indeed be exceedingly misleading. But, whatever theory may be formed of the actual nature of electricity, one thing is certain—that it is a medium for the transmission of force or power, and it is as such that I propose to deal with it this evening.

I take it, as one of my first articles of faith, that the conservation of energy is at all times a correct and reliable law, and that force, like matter, is indestructible; and when that force is converted from one form into another there can be no real loss in the transformation. I have heard it stated more than once that when one deals with large currents, or when we increase currents transmitted from a certain point, there is an enormous loss contingent upon such increase. In what this loss consists of has never been clearly stated; but I presume that it has reference to a confiscation, as it were, of energy resulting from the increased demand. Upon this there ought not to be two opinions; and I think I shall be able to show that, however great may be the supply of energy from a single point, with proper appliances and means of conveyance,

there is no limit to the extension, without loss in the transmission. I have said that we are to deal with energy itself, rather than with electricity, and to look upon electricity as a mere medium for its transmission and distribution. But, before going further into the question of energy and its sources, I propose to deal briefly with the methods by which electricity can be conveniently and economically used for the production of light, in a form suitable for domestic purposes.

I have here before me an electric lamp. Diagram I. represents a sectional view of this lamp. The light results from the incandescence of a continuous conductor made of a filament of carbon. This conductor is of high electrical resistance, so that a comparatively small amount of electricity driven through it develops sufficient heat to produce incandescence. The filament is, as you see, enclosed in a glass globe from which all deleterious atmosphere has been as completely removed as possible. In the construction of this lamp many minor points of detail have given rise to considerable difficulty and great delay in bringing it to perfection. The idea of electric lighting by incandescence of a continuous conductor is, as you well know, by no means new, but certain requsites for an efficient lamp had severally to be dealt with before it could be called practical. I refer to a lamp of small power, working at the same time economically. The first of these requisites is a suitable material out of which to form the incandescent conductor. It must have a high specific resistance, and expose but a small surface for radiation; next, it must be capable of enduring a high state of incandescence for an indefinite length of time without deterioration; next, the lamp must be easily constructed, and at a small cost. All these conditions are, I think, fulfilled in the lamp before you.

People are often surprised at the comparatively slow progress made in this branch of electric lighting, everything seems so simple; but it must be borne in mind that most things in connection with the subject are so exceedingly new, that it opens up, as it were, an entirely new industry; and it is not at all easy in a short space of time to get together a sufficient amount of intelligent labour for any very rapid progress. However, I may safely say

we have now made a fair start, and that, with a proper expenditure of energy on the part of those who are working at the subject, electric lighting for domestic purposes will, before long, not only be proved to be practicable, but also an accomplished fact.

In the diagram of the lamp, the curved line (A) represents the luminous conductor, or, as I sometimes term it, the luminous bridge. I have tried very many different materials for the purpose, all with more or less success. Any form of carbon, I find, will continue in a high state of incandescence without deterioration, provided that it is properly mounted and enclosed in a good vacuum. There is a certain temperature, however, above which the carbon bridge is rapidly destroyed. This temperature, which may be called the safe point of the lamp, is about 4,000° Fah. I have found that the most conveniently practical method of preparing a suitable carbon filament is by baking a good cotton thread or string in a hermetically sealed vessel, at a white heat. Conductors may be made in this manner with a resistance of several thousand ohms for only a few inches length.

In order to reduce this resistance, and to produce a harder and more durable carbon, I have adopted a plan first suggested, I think, by Mr. Sawyer, of New York, about three years ago, of heating the conductor to incandescence while immersed in a hydrocarbon liquid or gas; but, as I have said, this process is not by any means essential. Referring again to the diagram, LL are carbon cylinders into which the ends of the carbon filament are securee. A small hole is drilled right through these cylinders, and into these holes the ends of the carbon thread fit tightly. I have found that a very good cement for fixing these carbons may be made out of plumbago and Indian ink. The platinum wires (BB), which are about $\frac{3}{4}$ in. long and the $\frac{1}{100}$ of an inch in diameter, are fused into the glass tubes (CC), out of which they project into the globe about $\frac{3}{16}$ of an inch. The carbon cylinders (*ll*) are secured to these projecting ends in the same way as to the carbon filament. The other ends of the platinum wires are surrounded with mercury at CC, into which dip also the copper conductors (EE). The upper part of the lamp is entirely enclosed from the outer air by the glass blower.

The lamp is exhausted through a tube sealed off at F. Diagrams II. and III. represent the exhausting apparatus. The apparatus shown in Diagram II. is intended to work with sulphuric acid; III., mercury and sulphuric acid. It will not be necessary to go fully into the action of the pump: suffice it to say, that the liquid is forced up and down in the bulb (c); the rod (K), acting as a valve, opens and closes the neck of the bulb when required.

The following is a table showing the resistance of the carbon filament for a lamp of 15 candle-power for different degrees of electro-motive force. Each lamp of 15 candle-power absorbs about $\frac{1}{10}$ of a horse-power, and exposes on its "bridge" about the 10th of a square inch of luminous surface. I should mention that the resistance of a carbon filament is about 45 per cent. less while in a state of incandescence than when cold.

Electro-motive Force in Volts.	15 candle-light luminous bridge, having $\frac{1}{10}$ sq. in. radiating surface, absorbing $\frac{1}{10}$ H.P., has a resistance in ohms of	7½ candle-light luminous bridge, having $\frac{1}{20}$ sq. in. radiating surface, absorbing $\frac{1}{20}$ H.P., has a resistance in ohms of	5 candle-light bridge surface = $\frac{1}{30}$ sq. in.; HP. absorbed = $\frac{1}{30}$ resistance in ohms.
30	11·95	23·90	35·85
40	21·25	42·50	63·75
50	33·21	66·42	99·63
60	46·77	93·54	140·31
70	65·10	130·20	195·30
80	85·03	170·06	255·09
90	107·62	215·24	322·86
100	132·87	265·74	398·61
110	160·77	321·54	482·31
120	191·3	382·6	573·9
130	224·55	449·10	673·65
140	260·43	520·86	781·29
150	298·96	597·92	896·88
160	341·	682·	1,023·
170	384·08	768·16	1,152·24
180	430·5	861·	1,291·5
190	479·67	959·34	1,439·01
200	531·49	1,063·98	1,594·47

I will now deal somewhat in detail with the method by which I propose to distribute power, by means of electricity, from central

works, in the same way and very much for the same purpose that gas is distributed from gasworks over a large area. Diagram IV. represents the arrangements for distribution. C is a central station at which the electric current is generated. M is an electric main, branching in every direction, and wherever light is required. L are lamps in various places: one pole of these lamps is always in connection with the earth (E). W is a switch by which the electricity can be turned on from the main. This system of distribution will recommend itself to you, I think, as being simple and manageable. Assuming that the proper supply is always at hand, the light will always be produced by the mere turning of the handle of the switch or tap connecting the lamp with the main. These lamps being arranged in multiple arc between the main conductors and the earth, every lamp is completely independent of the rest; and, provided that the electric pressure is always constant, the resistance of the lamps being also constant, you may be sure of always having an equal amount of light when they are turned on, however variable may be the number at work in a given time.

The manner in which I keep the electro-motive force of the mains constant is as follows:—An electro-magnetic apparatus, which may be called an electro-dynamometer, and represented in Diagram V., is connected with the main, so that its electro-magnet (M) is constantly energised by a current flowing through it to the earth. This electro-magnet has a very high resistance, so that the current through it is very small. Now, the attraction of the armature (A) will evidently vary with any variation of the electro-motive force of the mains, however slight it may be. L is a lever, pivoted at C, and in connection with the mains. *h* and *k* are contact pegs, communicating by means of the wires *h′ k′* with the electro-magnets H and K in Diagram VI. or VII. The tension spring (T) exactly balances the attraction of the magnet on the armature (A) at the normal electro-motive of the mains. It is obvious that the slightest increase or diminution of the electro-motive force will bring the lever (L) in contact with one or other of the pegs (*h*, *k*), and so put into action one or other of the electro-magnets (H or K) (in Diagram VI. or VII.)

Diagram VI. represents an apparatus which will actuate a regulator or rheostat, which requires but little power to move it; whereas the machine shown in Diagram VII., which comprises an oscillating armature and ratchet-wheel, will afford power enough to operate a large throttle-valve or the expansion gear of an engine, or, in fact, any suitable regulator of the motive power. Should, therefore, the electro-motive force fall from an increased number of lamps being put into operation, the magnet (H) will be put into action, and an increased supply of steam will be allowed to pass through the throttle-valve of the engine, or will otherwise affect the electric generators, so as to increase their action. The reverse action takes place when the electro-motive force rises, owing to a number of lamps being turned off. By this means the power generated at the central station is made commensurate with the draught on the mains.

As to the size of the conductors required for any number of lamps, it should of course depend both on the number of lights they are intended to supply and on the distance from which the electricity is to be conveyed: there is of course another consideration—that is, the resistance of the lamps themselves and the consequent electro-motive force required to work them. Now, taking the resistance of each individual lamp of, say, 15 candle-power to be 250 ohms, it would be possible to work half-a-dozen lamps in multiple arc, supplied by a wire of No. 16 gauge, leading from a station $\frac{1}{4}$ mile off, without losing in transmission more than $\frac{1}{10}$ part of the total energy conveyed—that is to say, that the conductivity of the supply main would in this case be ten times greater than the conductivity of all the six lamps put together. A conductor of twice the sectional area would of course supply twice the number of lamps with the same proportional loss in conveyance.

Speaking generally, it may be said that if the resistance of the conductors or conducting mains, taken as a whole, be much less than the resistance of all the lamps taken as a whole, the loss of energy in transmission would be small in proportion.

It is quite feasible to construct lamps having twice this resistance, namely, 500 ohms each, or even more. Another point to be taken into consideration is this—that electricity could be con-

DYNAMOMETER.

Fig 3.

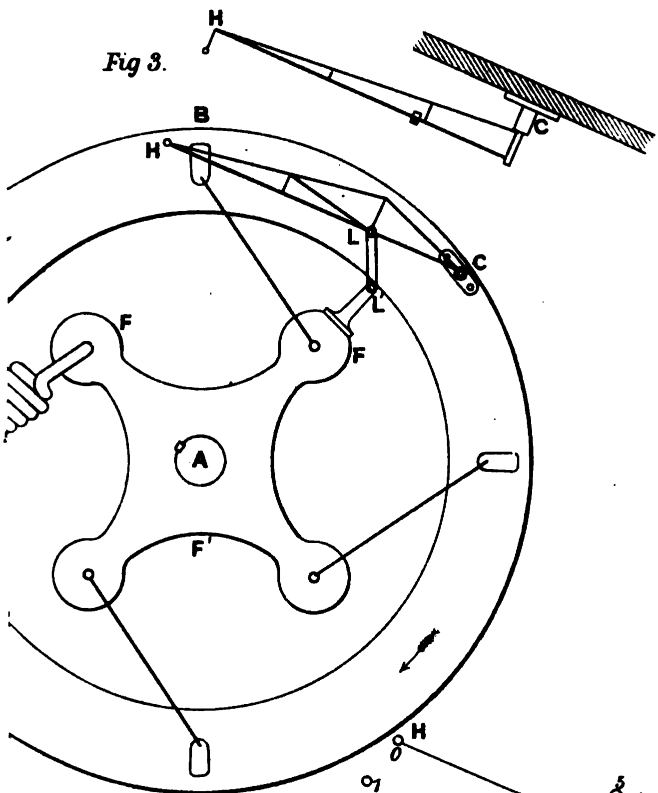
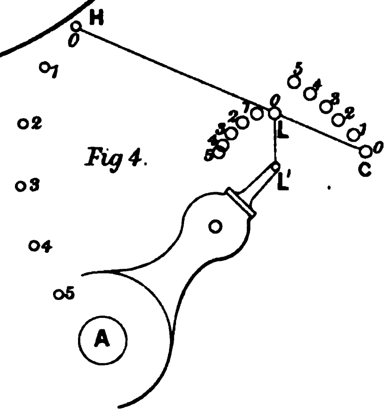
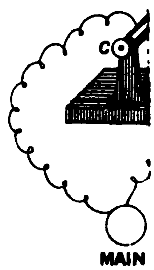
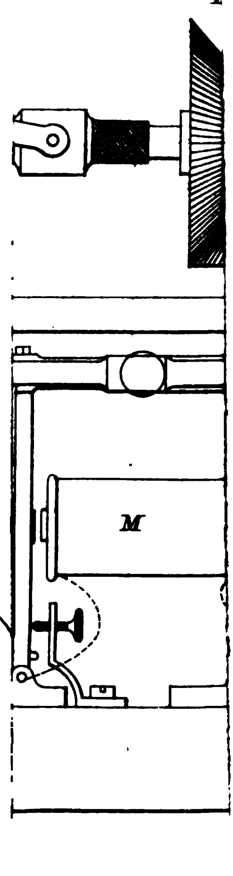


Fig 4.




C
MAIN


M

veyed from a very great distance at an enormous electro-motive force, which electro-motive force could be reduced to any extent by means of a system of condensers or of secondary batteries, and a suitable arrangement of commutators working automatically so as to suit the requirements for distribution.

It has often been argued that electricity cannot be stored like gas; but it should be remembered that electricity is not a fuel like gas, and that energy can be stored potentially in many practical ways, and that potential energy can be readily converted at any time into a kemetic form.

The question of economy of electric lighting is one about which there is more dispute and difference of opinion than on any other point. In dealing with this question, I prefer to look to the future, and to deal rather with what is possible and probable than with what has actually been done; for I hold that, unless we discuss a question on its future bearings, and put together facts arrived at from practical experience, with a view of ascertaining future results, there would indeed be no progress at all. I look, then, to the possibility of electricity works, so to speak, on a very large scale—on a scale comparable to that of gas-works of the present day, and I conceive an immense number of electric lamps operated from one point or station.

f once this point is conceded, I think that it can be clearly s. wn that electric lighting will compare in point of economy very favourably with any existing mode of producing artificial light.

With such a lamp and system as I have described, it is evident that the chief cost would be in the source of power. Now it is well known that, by the best steam-engines of the present day, power can be generated on a large scale by the consumption of 1½ lb. of coal an hour for every horse-power generated. From this basis I have calculated that power would not cost so much as ¼d. (one farthing) an hour per horse-power, and that, therefore, even for heating purposes, electricity would compare favourably with gas. It would obviously be impossible for me to fully discuss the whole question of economy at the present time, but I hope I have succeeded in showing that there is at any rate a fair basis to work upon.

The PRESIDENT: Before discussing Mr. Fox Pitt's interesting communication, I will ask Professor Ayrton to give us two communications by Mr. Perry and himself, and which are intimately connected with the subject of electric lighting.

A PORTABLE ABSOLUTE GALVANOMETER FOR STRONG CURRENTS,

AND

A TRANSMISSION DYNAMOMETER.

By Profs. W. E. AYRTON and JOHN PERRY, Members.

The object of the present communication is to bring before you a small dead-beat galvanometer which we have designed, and which Messrs. Paterson, of Little Britain, have been good enough to construct for us. This little portable instrument can be used for measuring the strongest electric light current directly in webers, without calculation or reference to any table, and the accuracy of its readings can at any moment be checked by the employment of only one single Daniell's cell.

In order that you may realise the advantages we believe this instrument to possess over other galvanometers previously employed for measuring very strong currents, it will be necessary to say a word or two about its antecedents.

A galvanometer, to be something more than a mere galvanoscope, must, of course, be calibrated—that is, the relative strengths of the current, producing different deflections must be ascertained. In certain cases, as, for example, with the tangent and sine galvanometer, this relative calibration can be effected by mathematical calculation; usually, however, it must be done by experiment. One well-known method consists in sending a current simultaneously through a standard galvanometer and the galvanometer to be calibrated, and varying the current in any convenient way. A number of simultaneous readings of the two galvanometers being taken, a curve is constructed, having for abscissæ the observed deflections on the galvanometer under test, and for ordinates the relative values of the currents, and from which the relative strengths of the currents producing any two deflections may be seen

at once. When, however, a standard galvanometer is unattainable, a current may be sent through the galvanometer to be calibrated, various known resistances being simultaneously put into circuit; then, as the current is inversely proportional to the resistance, the relative strengths of the currents corresponding with any two deflections may be easily calculated. A third method consists in always bringing the needle or movable part of the apparatus back to the zero position, by the introduction of a variable measurable force, in which case, whatever be the shape or size of the apparatus, the strength of the current can always be determined from the return force employed.

But for a galvanometer to be employed for the practical measurements of electric light currents, we must know not only the relative strengths of the currents producing any two deflections, but also the absolute value in webers of one of them: we must not only have the galvanometer *relatively* calibrated, but also *absolutely* calibrated. If our galvanometer be a tangent one, the absolute value of a current can, as is well known, be ascertained from the number of turns of wire in the coil, and its linear dimensions, provided also that we know the absolute strength of the horizontal magnetic field in which the needle moves. Now, although the strength of the earth's magnetic field in London is known with great accuracy, the actual field in a workshop is so modified by the iron machinery in the neighbourhood that we cannot, from the published tables of magnetic force, even approximate to the strength of the magnetic field in which our experiments are made.

The method, therefore, usually followed is to observe the deflection produced on the galvanometer when an electro-motive force of known strength in volts sends a current through a known resistance—that is, observe the deflection produced by a current of a known number of webers. When the magnetic field of the instrument is much affected by its position relatively to the iron in the neighbourhood, such a determination of the absolute value of a particular deflection must be made just before or just after an unknown current is measured.

When weak currents have to be measured, this is simple enough; but when it is large electric light currents that we have

to deal with, it is much more difficult, since if we attempt to produce with batteries and resistance coils currents having accurately a strength of even 50 webers, we run great risk of heating the resistance coils, and so making our current much less than we intend it to be; and it has been, I understand, this difficulty of constructing and using special resistance coils of very thick German silver wire, etc., that has delayed Messrs. Bréquet in the calibration and issue of the very convenient Deprez galvanometer that they have made.

If the galvanometer coil be large, then, whatever its shape, this difficulty is sometimes overcome thus:—Make a coil of fine wire to occupy, as nearly as possible, the same position as a single convolution of thick wire, and send a weak current of known strength through the fine wire coil. Suppose that a current of one weber circulating 30 times round the fine coils produces a deflection of 20, then we know that an electric light current of 30 webers circulating once round the thick coil will produce the same deflection: hence, with only the employment of weak currents, the instrument may be absolutely calibrated.

But such a form of instrument has two objections: the one that its large size makes it unportable, the other that it is not dead beat—that is, if there be a sudden change in the current, the needle is set swinging, and measurements of changing currents cannot be made. For the needle to be dead beat, its moment of inertia must be very small, and the magnetic field restraining its motion must be large.* But if a strong controlling magnet be attached to the apparatus, the coils must be made much smaller, or it will be very unsensitive. When, however, the dimensions of the instrument are small, we cannot even approximately make the fine wire coil through which the weak current passes occupy the same position relatively to the needle as is occupied by the thick wire coil through which flows the electric light current: hence the method of constructing the galvanometer with a thick and a thin wire coil cannot be employed for the object of absolute calibration.

Under these circumstances, a device that has been adopted is to add to a galvanometer with many convolutions, through which

* This principle has been applied for some years with great success by Sir William Thomson in his "Speaking Galvanometer" for submarine cables.

alone the weak calibration current passes, a shunt of small and known resistance through which flows the greater part of the strong current; but there is great danger of the resistance of the shunt being considerably increased by the heating produced by the strong electric light current, and an error consequently made in the measurement, so that this simple method cannot be recommended.

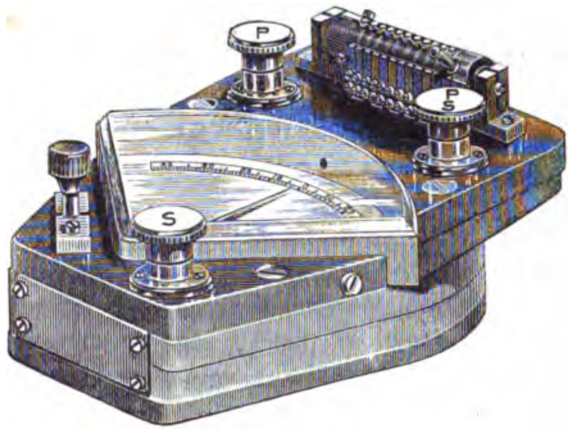
When a strong controlling magnet is attached rigidly to the apparatus, or when it is a spring that opposes the motion of the movable part of the apparatus, this absolute calibration need only be occasionally repeated to detect any change in the permanent magnet or in the elasticity of the spring. But such checks must not be neglected altogether, since it is well known that permanent magnets lose their magnetism, and that both magnets and springs are affected by changes of temperature.

And zero instruments, like the neat electro-dynamometer of Messrs. Siemens, although very convenient, have the objection possessed by a sine galvanometer of being unable to measure the strength of sudden waves in the current; also, they cannot be made dead beat on account of the comparatively large moment of inertia of the suspended coil. Now the recent experiments of the effect of whistling, singing, or the more unpleasant hissing of the electric arc, show the importance of an electric light current galvanometer being able to measure sudden short variations in the strength of the current.

We want, then, a small dead-beat galvanometer to use with strong currents, but which may be calibrated absolutely with a single Daniell's cell. As we did not know of the existence of such an instrument, we have designed for the students in the Course of Electric Lighting at the City and Guilds Laboratory, Finsbury, two instruments of this class on distinct principles, one only of which we shall bring before your notice this evening.

The small instrument before you, and shown half of full size in the accompanying woodcut, is very dead beat, this result being attained partly by the lightness of the needle and pointer, and partly from its moving in a very strong permanent magnetic field. The needle is balanced, and consequently the deflections are about the same for any position of the instrument. By a

proper arrangement of the coils, we have succeeded in making the deflections directly proportional to the current, and in the instrument on the table one degree deflection is produced by a current of two webers, the greatest deflection, 45°, being produced therefore



by a current of 90 webers. The main peculiarity of the instrument, however, is the following :—The thick wire coiled round the needle, and through which the electric light current circulates, is in reality a strand or little cable composed of ten insulated wires. Each of these wires having the same resistance, equal portions of the strong current flow through each of them. To produce a deflection even of 5° requires, as we have stated, a current of 10 webers, but by means of this simple commutator these 10 wires, which have hitherto been joined in multiple arc, can, by a mere turn of the hand, be connected in series, and now a current of exactly one-tenth part, or one weber, will produce 5° deflection. To ascertain, then, the real value of any deflection, all we have to do is as follows :—Turn the commutator to series, and send a current from a single cell—a Daniell or a Grove—of which the electro-motive force, E, but not necessarily the resistance, is known. A certain deflection a° is produced. Now take out the plug of the one-ohm resistance coil attached to the instrument, and a deflection b° is obtained. Then the resistance of the instrument, the wires, and the cell is

$$\frac{b^\circ}{a^\circ - b^\circ} \text{ ohms,}$$

or the deflection $a°$ in the instrument is produced by

$$\frac{E(a° - b°)}{b°} \text{ webers}$$

when the wires are joined in series, or by

$$\frac{E(a° - b°)}{b°} \times 10 \text{ webers}$$

when in parallel circuit.

Example.—With a Grove's cell you see that a deflection of 7°·4 is obtained on the instrument before you when the coils are in series, and 4°·1 when the one-ohm resistance is inserted. The resistance, then, of the coils, the connecting wires, and the galvanometer $\frac{b°}{a° - b°} = 1{\cdot}24$ ohms; or as the electro-motive force of a Grove's cell is 1·8 volts, a current of $\frac{1{\cdot}8}{1{\cdot}24}$, or 1·45 webers, produces a deflection of 7°·4, or one weber will produce about 5° with the coils in series, and therefore ½° with the coils in multiple arc. Consequently, currents from 0 to 9 webers can be measured when the coils are in series, and from 0 to 90 webers when in multiple arc, without any calculation or reference to tables, since the coils have such a form that the current is proportional to the angular deflection.

While, then, with the ordinary instruments employed for measuring strong currents, the absolute value of any deflection can only be checked by employing a current of known strength, and as strong as the one the intrsument is employed to measure, with the instrument before you the strong current necessary to produce any given deflection when the coils are in multiple arc is exactly ten times as strong as the comparatively weak and easily producible current of known strength which produces the same deflection when the coils are in series.

To render it impossible for the electric light current being passed through the coils when in series, the binding screws, marked P P, and to which the wires from the dynamo machine are attached, are only in circuit when the commutator is turned to parallel; the common screw, marked also S, and the third screw, marked S, being only in circuit when the commutator is turned to

series. Neither the coils of the galvanometer nor the one-ohm resistance coil, then, can be damaged by the commutator being left accidentally in a wrong position.

A TRANSMISSION DYNAMOMETER.

We now come to a dynamometer of another character, viz., one for measuring the horse-power transmitted by a shaft to any machine. Much of the vagueness which exists as to the economy in electric lighting has arisen from there not having been any easy mode of measuring the power absorbed by the dynamo machine. In the very complete investigations of Mr. Schwendler, Dr. Hopkinson, and of others, transmission dynamometers were of course employed, but they were laboratory instruments. You do not find such transmission dynamometers fitted up, almost as a matter of course, in all cases between the engine and dynamo-electric machines; and one reason of this is because such transmission dynamometers are separate machines occupying space and liable to a considerable amount of oscillation when in use, requiring therefore the employment of a dash pot to damp the vibrations, &c. Feeling the inconvenience of all strap dynamometers, we have designed for the Guilds Laboratory at Finsbury a form of dynamometer which can be permanently attached to any shaft, and which will show at a glance the force being transmitted to any particular machine—not a dynamometer that is specially fitted up with a certain amount of trouble for some special investigation, but an apparatus that is always ready and always indicating, whether or not it is desired to make any use of its indications. And as there may be others like ourselves who feel the importance of having some such instrument, which shows at a glance to a manager of works the power absorbed by any machine, we have thought you might be interested in seeing the working drawing of the dynamometer now being constructed to transmit 10 horse-power.

$B\,B^1$, figure 1, is a loose pulley used to drive, by means of a strap, any machine, such as a dynamo-electric machine. $F\,F^1$ is a boss keyed on the shaft driven by the steam engine: this boss has four stout arms seen in figure 3, which are attached by four spiral

springs to the rim of the loose pulley. If, then, the shaft be driven, and the motion of the loose pulley be resisted, by its having to drive, say, a dynamo-electric machine, the springs will be stretched, and the amount of such stretching, combined with the speed of rotation, will measure the power transmitted. So far we have been merely following General Morin. Now the question is how are we to measure the stretching of the springs—that is, the twist the loose pulley and the keyed boss undergo relatively to one another. Such a twist of course produces a tangential motion which we desire to observe without stopping the machinery, and consequently without any system of unrolling paper and moving pencil such as General Morin employed. We have sketched out various devices, in some of which the tangential motion was transformed into a radial motion, in others into an axial motion—that is, a motion along the shaft. For example, we employed the plan of attaching a cord to the rim of the loose pulley, and passing it over a little wheel on the arm of the keyed boss, its other end being attached to a little spiral spring which again was attached to the shaft. If, then, the loose pulley and keyed boss received a twist relatively to one another, the little spiral spring would obviously be more or less stretched according to the direction in which the twist was made. A bright bead then attached to any point on the radial portion of the cord would describe a larger or smaller visible circle as the shaft spun round, and the radius of such a circle would be a measure of the force transmitted. Another plan we employed was to carry this cord under a second little wheel on the shaft, and attach the end of the cord to a collar moving loosely along the shaft, the little spiral spring which previously was in the radial portion of the cord being now placed along the shaft, and having one of its ends attached to the collar, and the other to the shaft, so as to resist the motion of the collar produced by the pull of the cord. The force transmitted would then be measured by the position of this collar on the shaft. The method, however, shown in the diagram, of converting the tangential motion into a radial one, we consider preferable to the others just alluded to. H C is an arm, which, to combine considerable stiffness with lightness, is trussed in the plane of rotation as well as in a plane at right angles to this,

as seen in the upper part of figure 3. This arm turns about C, a pivot on the rim of the loose pulley, and is moved by means of a link, L L^1, attached at L by a pivot to the arm, and at L^1 by a pivot to the end of one spoke of the keyed boss, F F^1. If now the loose pulley and the keyed boss receive an angular twist, the end, H, of the arm, to which a bright bead is attached, moves almost radically towards the shaft. The exact path of the bead H is shown in figure 4 for different positions of C. Supposing, for example, we regard the keyed boss at rest, and that C moves to the point marked 1, then L will also move to 1 in the other curve, and H will move to 1 in the path it describes.

To measure, then, the power transmitted by the engine to the machine, all that is necessary is to observe on a scale placed in a suitable position, the radius of the circle traced out by the bright bead H, and the number of rotations made per minute, both of which can, of course, be ascertained at any time without stopping the machinery.

An alternative arrangement which we employ with very stiff springs is to have a small pinion at L^1, which works into a short spur segment on the pulley. A very small relative angular motion of F F^1 and B B^1 produces in this case a great angular motion of the pinion, and therefore of a light arm which turns with the pinion, at the end of which there is a bright bead.

It will be observed that this form of transmission dynamometer is not only extremely convenient when the dynamo-electric machine is driven by a strap, but will be especially suitable when the machine is driven direct from the shaft of a fast speed engine, as, for example, one of Messrs. Brotherhood's three-cylinder engines. In such a case the shaft would really be in two portions, end to end, supported by suitable brackets; one keyed boss would be on one portion of the shaft, and an arrangement something like the loose pulley we have referred to would be on the other; but in this case it would, of course, be keyed to its portion of the shaft. Motion would be communicated from one portion of the shaft to the other through the spiral springs, and the extension optically observed in the manner we have described.

The PRESIDENT then invited discussion on the paper of Mr. Fox Pitt and on those of Professors Ayrton and Perry.

Mr. DAVID CHADWICK: In order to commence the discussion, I should be very glad, as one of the public not professing to have much technical knowledge, first of all to congratulate Mr. Lane Fox and the Society for the very interesting lecture we have had. But there are many points, I think, that Mr. Lane Fox has omitted to mention. The public are looking with great interest to all Mr. Lane Fox's inventions, and to the inventions of every one connected with electric science. It is only a very short time since the newspapers were full of the discoveries of Mr. Edison, in regard, first, to the production of the incandescent light; next, the division of the incandescent light; and next, the invention of a lamp that should bring the incandescent light into practical operation. What I should like to know is this: What for practical purposes would be the cost and benefit of the incandescent light? The most interesting part of the lecture to-night, and of the exhibition to-night, is the small lamp applicable for domestic purposes. I should like Mr. Lane Fox, if he can, to tell us what would be the cost of introducing that light into a house of, say, £100 a year, and requiring, say, 10 lights; next, a house of £200 a year, and so on. What the public are interested in knowing now is, seeing that the electric light for street purposes and for the illumination of railway stations is an accomplished fact, Can the electric light, either by the inventions of Mr. Lane Fox, or of any one else, or all put together, make the electric light applicable to domestic purposes? It is quite clear that it can be brought to illuminate large rooms of this kind: we see that it can be brought to illumine the streets; but the lights for domestic purposes are the most important, and, to my mind, the most interesting invention which has been brought before us. I should like some member who follows me, after my very few remarks, to say what are the special advantages of the Lane Fox light in comparison with the Swan light, with the Edison light, and with the Siemens light? Have any other persons brought the light nearer to perfection for domestic purposes and for constant use than Mr. Lane Fox? Is the British Electric Light Company (who

have, I believe, fitted up the display to-night) prepared to give me an estimate of the cost of a gas-engine, either Otto's, Crossley's, or any other, and a generator which can be connected therewith, for illuminating my house in the country? We know that Sir William Armstrong has lit up his house on the Swan principle. Has any one done it on the Lane Fox or any other principle? and, if so, what are the advantages? Will Mr. Lane Fox or any other engineer present enlighten us on these matters? because all the inventions, as far as the public are concerned, are comparative. We want the best, the simplest, and the cheapest light; and if any one can make a lamp that is applicable at a cheap rate, or at anything approaching one shilling per lamp, the cost Mr. Lane Fox has stated as the cost of the lamp now exhibited, it will be greatly appreciated. Suppose we call it 5s., to include manufacture and profit; and if the cost, instead of a farthing per hour for a 150 candle light, is four times that amount, or one penny per hour, then I say a very great object has been accomplished. It will then behove all who are interested in gas companies to look about them. I should be very glad to hear some practical remarks by men who thoroughly understand the matter and can tell us what are the relative advantages of this beautiful light, than which I never saw anything more brilliant, more soft, and more applicable for domestic lighting. What are the comparative advantages of this with all other lights? As you have invited me and others of the public here, I venture to suggest that we do not want to go into the technical question, but to leave that for electric engineers to discuss. It is a great and interesting public question. I hope some one will follow me who will tell us how soon it can be applied generally to domestic use, and the cost in the cases to which I have referred.

Mr. CROMPTON: Mr. Chadwick calls upon our President to pronounce authoritatively which system of incandescent lighting (the author's, Mr. Swan's, or Mr. Edison's) he considers best suited for the requirements of private users of the light. I think, however, that he will hardly obtain the decisive opinion he wishes for, for several reasons. The above-named inventors are now running neck and neck in an honourable race to supply us with a truly successful light for domestic use, and nothing short of

extended practical application of the several systems will furnish sufficient data on which to found such decisive opimons as those asked for.

I have just returned from the Norwich Fisheries Exhibition, where we have the incandescent lights of Mr. Swan in use, to light up all the smaller spaces and offices. As far as mere outward effect is concerned, the results we show at Norwich are very similar to those before you this evening. Again, Mr. Chadwick's questions as to cost of maintenance cannot be answered for much the same reason, viz., want of extended practical experience. I think enough has been done, however, to show that we can effect a considerable saving over gas.

Referring now to the papers under discussion, I have to ask Mr. Lane Fox Pitt for information on several points. Has he considered that his plan of distribution, being simply the multiple arc system, slightly varied by the earth being used as the return circuit, is wasteful of current, as compared with those systems where a current of higher E.M.F. is used sufficient to work several lamps in series in each branch of the circuit? I made a calculation for Mr. Swan some time ago, which showed that, given an incandescent lamp of a resistance of 40 ohms when hot, and requiring a current of 1 weber to raise it to the required degree of incandescence, the energy required by each lamp is ·053 H.P., or rather over 18 lamps to the H.P. Now, by using currents of high tension, such as have already been successfully employed by the Brush Company, we can work such a large number of these 40 ohm lamps in series as to utilise 80 per cent. of our energy in the lamps, and thus get 14 lamps per H.P.; whereas if we use currents of low tension, and work the same lamps in multiple arc, we can only get 10 lamps per H.P. Of course I am aware that the author claims the merit of greater simplicity for the multiple arc arrangement, but this simplicity may be too dearly purchased at the above great loss in economic efficiency.

Turning now to the regulator by which the author proposes to keep the mains at a constant potential, I may remark that, so long as he uses lamps in parallel arc and self-exciting dynamo machines to produce his current, the machines themselves are self-regulating

within certain limits of E.M.F.: each lamp, as it is switched in or out of circuit, diminishes or increases the resistance, and consequently increases or diminishes the current almost in the exact proportion necessary to keep the current at a constant potential.

The electrical regulator shown by the author is extremely ingenious, but I doubt much whether it will have power to work anything but the throttle valve of a steam-engine; whereas the tendency of mechanical engineers for the past few years has been to do all the governing of the steam engine, by varying the cut-off of the slide valve—a duty which requires a governor of much greater power than does moving the throttle valve.

The Brush Company have, I am told, an extremely ingenious regulator, which controls the E.M.F. of their machines, by short-circuiting a portion of the fixed magnets. This appears a more fitting duty for Mr. Fox's regulator, than attaching it to the steam-engine direct.

As to the use of the secondary or reservoir batteries to act as a store of electrical energy, it is one of those things, much talked of, but never yet practically carried out. M. de Meritins told me a few days since that a secondary battery had been recently brought out in France, which gave surprising results: five or six times as much energy can be stored in it as in the Planté battery. In fact, if I understood him aright, 32,000 foot-tons of energy could be stored in the space of a cubic metre. This is something like an accumulator; and, if it be true, we have got our electric light "gasometer."

I am curious to hear the author's experiences in coupling up dynamo-electric machines in quantity to work circuits. So long as we use simple self-exciting machines, we have had considerable trouble with reversals of polarity, causing one machine to drive its current through the other one, instead of sending it through the external circuit. Of course, with separately excited or magneto machines, this is not the case.

One other point I find difficult to understand, if Mr. Fox is correct in his measurement of resistance of these lamps at 85 ohms when hot. If we take the current passing at one weber for lamps, we have 52 webers through a total resistance of about 2·5 ohms, say, 1·6 in lamps, ·7 in machine, and ·2 in leading wires. This is equal

to rather over 9 H.P. of actual electrical energy developed in the circuit. The B gramme is a very powerful generator of current, we know, still I don't think it will give off so much energy as this. I think, probably, the author has over-estimated the resistance of his lamps.

Professor Ayrton's galvanometer will be a great help to us electric light engineers: we are greatly in want of a trustworthy galvanometer of sufficient portability to be carried down to the various instalations as we fix them.

His transmission dynamometer is not new. A young electrician who is working with me, Mr. Harold Thomson, schemed out almost identically the same arrangement some months since, the difference being in the mode of observing the tension on the springs; the extension or compression of the springs causing a longitudinal movement of one of the pulleys along the spindle, which movement could be observed by a lever, the short end of which pressed lightly against the end of the pulley boss, the other end carrying a recording index.

Professor AYRTON: But it is this very mode of observing the tension on the springs that Mr. Perry and myself regard as the novelty, since, as I have already mentioned, spiral springs for dynamometers were used long ago by M. Morin.

Mr. MITCHELL: I was invited here, on behalf of a paper, to take part in this discussion; and, although I do not wish to follow the remarks made as to the question of cost of electric lighting, or to enter into matters of detail, there is one question which I should like to re-echo here: What are we to depend upon with regard to our electric light? When Mr. Preece lectured recently at the Society of Arts, he said that the engine had been too much overlooked: matters of detail between one light and another were frequently gone into; but the prime force we had to depend upon was the engine. This evening I have those remarks brought back to my mind by what I have just heard, and by what Mr. Preece said to Mr. Fox, that the accident to the light was due to the engine strap slipping. I think this is a favourable opportunity for obtaining the information which I have so frequently asked for: How are we to get over this difficulty of the strap slipping? If

there is an answer I shall be glad to know, as, if this Society can get over that, a great difficulty will be overcome. If we are going to introduce electric lighting, I do hope that it will be done simply and thoroughly; but, when I am asked this question, I want to know what answer I can give. Perhaps we shall have to go back to the old adage of the shoemaker, that there is nothing like leather; but I shall be glad to have information on the point.

Mr. C. G. Gumpel, A.I.C.E.: Sir,—There are two or three points on which I should like to have some information, which I think Mr. Lane Fox can give.

We have had this brought before us as a new system. I have failed to discover the difference between this and Mr. Swan's, which has been exhibited several times in London, and between Mr. Edison's system. In all these three cases it is a carbon filament which is brought to incandescence, and, so far as the outside public is concerned, we do not know the difference. Edison's system, no doubt, takes precedence in point of time, and also in the preparation of the carbon filaments, by exhausting the vessel while they are under incandescence, greatly improving thereby the vacuum, and increasing the durability of the filaments, which I believe is a great step towards success.

Mr. Lane Fox has not told us about the manner of preparing his carbons, and it will be remembered that Mr. Swan avoided the question; whereas the whole and chief difference between Mr. Fox and Mr. Swan is only in the production of the carbon filament.

Next, with regard to the arrangement of the lamps. It is well known that, if we put a number of these electric lamps in parallel circuit, when one or more are turned out, unless there is an equivalent resistance substituted, the machine will either be short-circuited and the armature be heated, or the carbon filaments in the lamps will be destroyed. To avoid this evil in the dynamo machine, the multiple circuit was abandoned, and the series arrangement with compensating resistances adopted, as has been stated by Mr. Crompton.

In regard to economy, it is a very delusive statement to make, that our steam-engines can be worked on 1½ lbs. or 1¾ lbs. of coal per horse-power. As a practical engineer, I know that

there are no such cases. The nearest approach to it is that of Mr. Loftus Perkins, and he, I think, approached it to a very small fraction under 2 lbs., 1·6 lbs. or 1·7· lbs. The greatest effect attained in steam-engines was in the "Iona" on the Clyde, which was a special favourite of Professor Rankine's. That vessel produced one horse-power with about 2 lbs. of coal, which is very rarely reached; and even in the largest engines, working up to 8,000 horse-power, it approaches 3 lbs. In ordinary steam-engines now working, the average consumption is 3 to 4 lbs., and the great point is this, as put by Mr. Chadwick just now: What is the cost of the electric light where large centres for distribution cannot be established? If I had a house in the country I would not listen to any one telling me the light could be produced by 1½ lbs. of coal per horse-power, but should want to know the price of an apparatus that would provide so many lights with an expenditure of 5 to 6 lbs. of coal per horse-power per hour. That is the figure which should be brought into calculation. The electric light is at present but little established: it is a thing which has to be developed; and under these circumstances few will adopt it unless they can afford a certain sacrifice and run a certain risk. Were I a capitalist, I would not hesitate to go to the expense, if only to promote it; but the ordinary public will not do this, and we must give them reliable figures, or destroy their confidence.

The apparatus by means of which Mr. Lane Fox proposes to regulate the dynamo machine through the steam valve, as already pointed out by Mr. Compton, is not up to the standard of engineering at the present time. To wire-draw the steam is not the best means of regulating an engine either for economy or efficiency: it must be by the slide valve; and the great advantage of the Corliss engine is simply owing to the fact that the steam is cut off as near as possible at the steam cylinder, to get the most beneficial result. Anything of the kind suggested by Mr. Fox would not be adopted by any engineer, and, even if his regulator were adopted, I am afraid it would be unsteady. Should the current be too strong, the regulator would cut off the steam: this would weaken the current, which then would turn the steam on again, and so oscillations would be the result. The regulation of the current is a most difficult task to

fulfil, and, as Mr. Crompton has stated, there are already better systems in vogue than the one now before us. Any practical engineer or electrician who designs a very efficient regulator will most decidedly promote the advance of the electric light.

On the whole, we can but thank Mr. Lane Fox for bringing his system before us. There is no question that, until the electric light can be introduced into our very houses, we shall not look upon it as a light to be generally adopted. But when we are asked about steam engines and gas motors for producing the light, let us give our friends reliable figures.

The further discussion on Mr. Fox Pitt's paper was then adjourned until April 28th, and the meeting concluded by a ballot, at which the following were elected

As Foreign Member:

Captain Andrés A. Comerma.

As Members:

John Oldham.

C. W. Sleeman.

As Associates:

John R. Andrew.

Charles Moseley.

F. J. Mudford.

George Frederick Rogers.

John Scudamore Sellon.

Lawson Stockdale.

Harold Lyon Thompson.

ORIGINAL COMMUNICATIONS.

THE THERMO-ELECTRIC NEUTRAL POINT.

By A. Partz.

While engaged of late in making a series of thermo-electric experiments, I had repeated occasions to fully convince myself that the electric currents resulted purely from a conversion of the applied heat. I also obtained a little more light on the conditions under which that conversion took place, and detected various errors in the statements of former investigators of the subject. For instance, there is evidently something wrong about the iron line in the diagram on p. 178 in the highly meritorious book of Professor Jenkin; indeed, the whole diagram seems to need a revision. This may be shown by a few simple trials. Take a platinum and an iron wire, each about 1 mm. thick and at least 15 cm. long (so that the heat be not conducted too readily to the pole connections), twist them together and hammer the twisted ends a little flat, so as to prevent them from untwisting when heated; form a circuit through a galvanometer, and introduce the twisted end gradually into a strong flame. You will see the needle steadily deflecting in the same direction until the wires have attained at their junction a bright red heat, when it suddenly stops, and it will then not move either one way or the other, even though the heat be raised considerably higher by the aid of a blowpipe; and, when you withdraw the couple from the flame, the needle will steadily recede to 0°. There is, then, no reversion of the current, no neutral point; but there is a point of maximum force. In making the experiment with wires of argentine and iron, and of argentine and copper, the same result will be obtained in a more striking manner, because of the greater strength of the currents; and here it will be found that the maximum force of the couple of argentine and copper is not much inferior to that of

argentine and iron. With couples of silver and iron, copper and iron, and aluminium and iron, I did observe a reversion of the currents, which went first from the former to the latter metals through their heated junctions, and then *gradually* turned the other way; but here also occurred the same points of maximum force beyond which the currents could not be pushed by an increase of heat.

These and other trials led me to conclude that I had got on the track of a new law, which may be stated as follows:—

"The thermo-electric current of a metallic couple attains its maximum strength when the molecular structure of one of the metals begins to change, at the heated junction, from that of solidity to that of plasticity, when the metal begins to take up latent heat."

To test this law at a moderate temperature, and in such a way that the change in the state of aggregation of one of the metals could easily be observed, I made the following experiment:—I cast bismuth in a porcelain tube 1 cm. in diam. and 16 cm. long, inserting at each end a copper wire, one passing through a plug of plaster. Having connected the wires with a galvanometer, I held the tube in an inclined postition, with its open end over an alcohol flame. The moment when the bismuth began to soften, so that the inserted wire became slightly loosened, the needle stopped advancing; and, although I then increased the heat far beyond the melting point of the metal (the lower end of the tube being placed in water), the needle remained at the same spot.

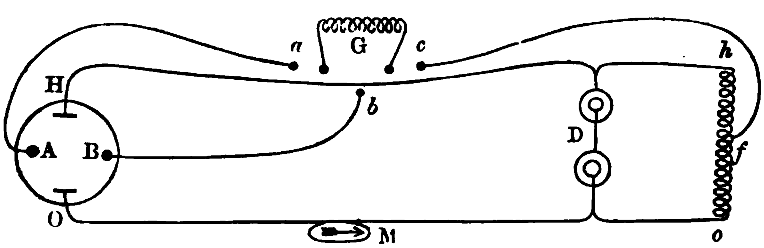
As regards the reversion of thermo-electric currents, I am confident that future investigations will prove them to be due to such molecular changes as are, with respect to some metals, already known to take place at comparatively low temperatures. Thus, while zinc is malleable at 150° C., it becomes brittle at 200°, and that change just coincides with the inversion of its current with iron.

ABSTRACTS.

H. HELMHOLTZ—ON CURRENTS FROM POLARISED PLATINUM PLATES.

(*Annalen der Physik und Chemie, B. XI., H. 5., 1880, No. 12, pp. 737-759.*)

The author was led, by his experiments on electrical capillary phenomena, to investigate whether similar results were caused by the motion of an electrolytic fluid along polarised platinum plates. It was necessary to observe the effects produced by the polarisation of one electrode independently of the simultaneous polarisation of the other. The experiments were made with electrodes of platinum wire (0·5 mm. thick, 60 mm. long), which was fused into glass tubes where it cut the surface of the liquid. These dipped into slightly acidulated water. In one series of experiments, where the platinum wire acted as kathode, the second electrode was zinc amalgam, which remained unpolarised. In many other series, the second electrode was a pair of platinum plates, between which a continuous weak current from two Daniells was maintained, sufficient to decompose water. Besides being connected by the two platinum plates in the water, the battery circuit was completed through a resistance of 2,000 Siemens units. By means of a sliding contact piece on this shunt circuit, connection could be made through a Thomson galvanometer with the platinum wire electrode. With this arrangement, the pair of platinum plates acted as an unpolarised electrode. The figure shows the connections, in which A and B are the platinum wires, H and O the pair of platinum plates for decomposing



water, D the two Daniell's cells, *o h* the scale of 2,000 units resistance, and G the galvanometer, which can be joined up to *a* and *b*, *a* and *c*, or *b* and *c*. According to the position of the contact piece (*f*), the E.M.F. could be varied from — 1,000 to + 1,000 in terms of the scale, and these values, divided by 1,086, give the E.M.F. in Daniells. If an E.M.F. = — 1,000 is allowed to act for several days, to get rid of all occluded hydrogen by the development of oxygen on the surfaces of the wires, and then the E.M.F. is varied from — 900

to 0, a decrease of the E.M.F. between these limits causes a temporary negative fluctuation of the current, an increase causes a positive one. But as soon as the limit E.M.F. = 0 is passed, strong, persistent, positive currents are produced. Since 1½ Daniells are necessary for electrolysis, no separation of free hydrogen can take place with the E.M.F. employed; and the author concludes that the great increase of the current is due to the taking up and occlusion of the hydrogen by the platinum. After the first strong current has abated, reduction of the E.M.F. produces an increase of the former current, after a short negative deflection. This is not observed if the platinum plates have become saturated with hydrogen.

In one instance an E.M.F. = + 1000 was allowed to act for a fortnight. The current fell gradually to less than half the strength it had at first. On making changes in the E.M.F. by steps of 100 S.U. at a time, every backward step gave a negative deflection, every forward step a positive one. On returning to negative E.M.F., continuous currents are produced which have much greater intensity than those first obtained. The cause of these currents is doubtless to be found in the circumstance that the occluded hydrogen gradually soaks out of the platinum, and combines with the oxygen set free from the electrolyte by the E.M.F.

In investigating the effect of currents of water along polarised platinum surfaces, the thin platinum wires above described were used, and were kept in vibration by slight taps with a glass tube; or the electrodes were placed at the openings of small tubes, from which water was allowed to flow into the large vessel (C). It is necessary to distinguish the primary current which is developed before the electrodes are shaken, and the concussion current due to the shaking. According as the shaken electrode is kathode or anode, the author terms the concussion-current kathodic or anodic, and he deduces the following laws from his observations:—

1. When there exists a strong kathodic primary current, the concussion currents are always in the same direction and strengthen the existing current.

2. When the primary current is anodic, or weak kathodic, the concussion-currents are anodic.

3. The covering of the surfaces of the platinum plates with hydrogen promotes the development of anodic concussion-currents. These are strongest if platinum plates strongly charged with hydrogen are brought under the action of anodic E.M.F. The limit between currents which give anodic and kathodic concussion-currents lies at weaker kathodic currents for platinum plates poor in hydrogen than for those rich in hydrogen.

4. If the primary current is interrupted, anodic concussion-currents are obtained, which are stronger the more the electrodes are charged with hydrogen.

A current produced by shaking of the electrodes, even after long duration, gives no deflection in the opposite direction, as do alterations of the current due to change of resistance or E.M.F. with polarised plates. It may be concluded that the concussion-current is not produced by any of the changes which would have produced the polarisation-current in a state of rest. A strong kathodic current removes the acid from the neighbourhood of the electrode, and forms

badly conducting layers. If these are washed away, an increased current must result. This causes the kathodic concussion-current; but in the case of anodic currents there is nothing corresponding to this cause.

In general, then, if the concussion does not alter the resistance, wires which contain much occluded hydrogen give kathodic concussion-currents; those containing less, strong anodic currents, and those with very little hydrogen, weak anodic.

The author proceeds to give his hypothesis, which is founded on the above experiments and others. He starts with the view that a different degree of attraction for the two kinds of electricity belongs to every substance which can act as a metallic conductor. Since, whenever positive electricity is produced, an equal quantity of negative is also called into existence, it is only necessary to speak of the forces acting on the positive. If the work which is performed by this attractive force, when the electrostatic unit of positive electricity passes from any metal serving as base of the series at a zero potential into the interior of a metal (M), is put $= G_m$, and the value of the potential in $M = \phi_m$, then there is electrical equilibrium between two metals (Z and C) when $\phi_z - G_z = \phi_c - G_c$. The constants (G), which the author terms the galvanic value, determine the place of the metal in the voltaic series. The zero of the series can be fixed arbitrarily, and the author chooses the metal of the electrometer which receives the charge from the body to be experimented upon, in which case $\phi_z - G_z$ and $\phi_c - G_c$ are the values of the potential.

To explain Faraday's law of electrolysis, he assumes that in every compound body capable of electrolysis, each combining unit of the kation is united with one equivalent of + electricity, each combining unit of the anion with one equivalent of negative. No other force (chemical) is opposed to the free motion of the positively and negatively charged ions, than their electrical attraction and repulsion. In order that a number of positive ions may separate electrically neutral and chemically uncombined, the half of them must give up their equivalents of + E, and take up the corresponding − E. This transference is accompanied by a considerable consumption of work, and constitutes the definite separation of the former chemical combination. If the electrolytic liquid is in contact with two electrodes of unequal potential, a collection of the atoms of the positive ion takes place at the negative plate, and of the negative at the positive plate, until the potential in the interior of the liquid has reached a constant value. If positively charged atoms collect along the outer side of the surface of the electrodes, the corresponding quantity of negative electricity is attracted to the inner side, so as to form an electrical double layer. In such a case molecular forces must act, which have a very small, but finite, sphere of influence; for otherwise the distance of the two layers from one another would be infinitely small, and the work of the electrical forces corresponding to the accumulation would be infinitely great. In the former case the double layer will possess a finite moment, and form, in fact, a condenser of extraordinary high-capacity. So long as no chemical action alters the quantity of the accumulated electricities, the potential of the liquid between the two electrodes is determined by equal

quantities of $+$ E and of $-$ E having accumulated at both electrodes. Let E be the quantity of the accumulated electricity, F_1 and F_2 the areas of the two electrodes, C_1 and C_2 the capacities of unit of surface (which, as far as possible, are functions of the thickness of the layer), ϕ_1, ϕ_2, and ϕ_0 the potentials of the two metal plates and of the fluid, then there is electrical equilibrium when

$$(1.)\quad E = F_1 . C_1 . (\phi_1 - \phi_0); \; E = F_2 \; C_2 . (\phi_0 - \phi_2).$$

$$(1a.)\quad \phi_1 - G_1 - \phi_2 + G_2 = A$$

where A = E.M.F. of the battery.

From this follow the equations, entirely establishing the conditions of equilibrium.

$$(1b.)\quad \begin{cases} E\left\{\dfrac{1}{F_1 C_1} + \dfrac{1}{F_2 C_2}\right\} = A + G_1 - G_2 \\ \phi_1 - \phi_0 = \left\{A + G_1 - G_2\right\} \dfrac{F_2 C_2}{F_1 C_1 + F_2 C_2} \\ \phi_0 - \phi_2 = \left\{A + G_1 - G_2\right\} \dfrac{F_1 C_1}{F_1 C_1 + F_2 C_2} \end{cases}$$

The processes explained by the author on this hypothesis are (1) the electrolytic separation of the ions from a liquid, and (2) the occlusion of hydrogen in the metal of the kathode.

Dr. F. FUCHS—ELECTRO-MOTIVE FORCE OF VARIOUS ZINC-COPPER ELEMENTS.

(*Annalen der Physik und Chemie, B. XI., H. 5, No. 13, 1880, pp. 795-800.*)

The current from several Bunsen's cells was sent through a voltameter, two rheostats, a platinum wire (a b), and so back to the battery. A shunt circuit connected with the points a and b of the wire contained a contact-key, a galvanometer, and the element to be experimented upon, the positive pole of which was connected up to that point of the wire (a b) at which the main current entered. The current strength in the main circuit was so regulated by means of the two rheostats, that the galvanometer needle remained at rest. The voltameter was put in connection with a glass globe for collecting the gas evolved for a time (t), which was generally thirteen minutes. If the galvanometer gives no deflection during this time, the difference of potential between the points a and b is equal to the electro-motive force of the experimental cell. The current strength in a b (resistance $= w$) is $I = \frac{E}{w}$; and since the current strength in the voltameter equals that in a b, if s is the reduced volume of gas, $I = \frac{s}{t}$; and hence $E = \frac{s \cdot w}{t}$, where s, is in cubic centimeters, t in minutes, and w is equal to 1·002 Siemens' units. The platinum wire was soldered at either end to a copper rod. The two rods were attached to, but well insulated from, a piece of cork; the whole being placed in spirits of wine, which

prevented any change of resistance due to heating of the wire. The resistance of the wire between the points a and b was determined by a modification of Wheatstone's method. One branch of the bridge contained the unknown resistance (a b), and a standard unit coil (c d), the other branch was a wire stretched over a scale. The bridge wire was connected successively to the points a, b, c, d, and the needle brought back to zero by shifting the point of contact with the stretched wire. In this way the points α, β γ, δ, were obtained, the potentials of which equal those of a, b, c, d respectively. The resistance a b has the same ratio to the standard c d, as the length α β to γ δ. The results obtained were a b = 1·002, 1·002, 1·001 Siemens' units. The globe for receiving the gas was calibrated in cubic centimetres by weighing with mercury: and during the experiments it was surrounded by a water bath, the temperature of which was taken by a thermometer reading to fifths of a degree. The strength of the current was brought to the desired point by means of the two rheostats; the first an ordinary one, and for finer adjustment a second, consisting of two platinum wires fixed by corks in a tube, which could be immersed more or less in a test-tube of mercury by means of a screw, thus allowing a greater or less length of the wires to be brought into circuit. The numbers in the following tables are the volumes of gas in cubic centimetres produced in one minute by one element as described, working through a total resistance of one Siemens' unit. The figures in brackets show the parts by weight of the salt in the solution.

Cell I. Amalgamated zinc; zinc sulphate (1—3); copper sulphate (1—5); copper.

Exp. 1. E = 12·21; 12·15.
3. E = 12·14; 12·12.
5. E = 12·19; 12·20; 12·18.
7. E = 12·23; 12·19.
9. E = 12·24; 12·21; 12·28; 12·25.

Mean, E = 12·2.

Cell II. Amalgamated zinc; zinc sulphate (1—3); copper sulphate (1—4); copper.

Exp. 16. E = 12·18; 12·15.
17. E = 12·18; 12·18.

Mean, E = 12·18.

Cell III. Amalgamated zinc; sulphuric acid (sp. gr. = 1·131); copper sulphate (1—5); copper.

Exp. 2. E = 13·11; 12·84.
4. E = 13·10; 13·07; 13·05.
6. E = 13·10; 13·08; 13·05.
8. E = 13·22; 13·23; 13·14.
10. E = 13·22; 13·08.

Mean, E = 13·15.

Cell IV. Amalgamated zinc; zinc sulphate (1—3); copper sulphate (1—2) copper.

Exp. 11. E = 12·52; 12·46
12. E = 12·32; 12·32.
13. E = 12·40; 12·38.
Mean, 12·41.

Cell V. Same as above, but 2 volumes of the copper solution diluted to 5.

Exp. 14. E = 11·90; 11·92.

F. EXNER.—THE THEORY OF VOLTA'S FUNDAMENTAL EXPERIMENT.

(*Annalen der Physik und Chemie, B. XI., H.* 5, *No.* 13, 1880, *pp.* 1034-1036.)

The author has shown in the *Annalen*, B. IX., p. 591, that the causes of difference of potential are not to be sought for in the contact of two metals, but in chemical changes. He shows by one experiment chosen from five that no difference of potential arises from the contact of two metals. Both pairs of quadrants of an electrometer were connected with the poles of a Zamboni's pile, the needle with one of the plates of a zinc-zinc condenser. On connecting the second plate of the condenser to the copper pole of a Daniell, the zinc pole of which was to earth, the electrometer gave a deflection = + A. Discharging the whole apparatus and reversing the connections of the Daniell, the electrometer gave a deflection = — A. (A was = 18 mm.) According to the contact theory (if E means earth, and F, fluid in the cell),

$$E \mid Zn + Zn \mid F + F \mid Cu - Cu \mid Zn = +A$$
$$\text{and } E \mid Cu + Cu \mid F + F \mid Zn \qquad = -A$$
$$\text{and therefore } E \mid Zn + E \mid Cu + Cu \mid Zn = 0 \qquad \text{(1)}$$

Replacing the zinc condenser by a copper one, and repeating the experiment,

$$E \mid Cu + E \mid Zn + Zn \mid Cu = 0 \qquad \text{(2)}$$

But from (1) and (2) $Cu \mid Zn = Zn \mid Cu$, that is, $Zn \mid Cu = 0$

The author considers that Volta's experiment is a phenomenon of static induction. The charge of a zinc-copper condenser commences already on approaching the plates, and therefore without any contact. This induction seems to have its source in the electrical films of oxide which cover the metals, and which in different metals possess different potentials corresponding to the heat of oxidation.

F. EXNER.—THE THEORY OF THE GALVANIC CELL.

(*Annalen der Physik und Chemie, B. XI., H.* 5, *No.* 13, 1880, *pp.* 1036-1038.)

The author having shown in the above article that no electro-motive force is produced at the point of contact of two metals, investigates the action of the galvanic cell from a chemical point of view. A condenser, with one plate of zinc and the other of water, behaves as a zinc-platinum condenser, while a

copper-water condenser behaves like one of copper and platinum, which shows that water plays the same part as platinum. The author experimented with a condenser, one plate being zinc and the other various liquids, the connection being a platinum wire. The liquids taken were water and solutions of $H_2 SO_4$, $Cu SO_4$, $Zn SO_4$, HCl., HNO_3 and alcohol, and he found that the charge remained always the same. Since, then, where there is no chemical action there is no production of electricity, there can be no longer any doubt that the electrically active points are those where such action does exist. It by no means necessarily follows that the free potential at the poles of an open cell are equal; their ratio depends on the capacities of both poles: if these are equal, then the potentials are equal. If S is the constant difference of potential of the poles, C the capacity of the positive pole, c that of the negative, then—

$$\text{Free potential at} - \text{pole} = -S \frac{C}{C + c}.$$

$$\text{Free potential at} + \text{pole} = +S \frac{c}{C + c}$$

On investigating the course of the potential in open and closed cells, it was found that a change occurred only at those points where there was chemical action. Thus in a Smee's cell it occurs between Zn and $H_2 SO_4$, not between $H_2 SO_4$ | Pt, nor naturally between Pt | Zn. In two-fluid cells, *e.g.*, Daniell's, the potential suddenly alters at two limits, viz., Zn | $H_2 SO_4$ and $H_2 SO_4$ | $Cu SO_4$. At the former limit Zn is oxidised by reduction of H_2, at the latter H_2 is oxidised by reduction of Cu; and in both cases the change of potential corresponds to the heat equivalent. A Daniell may be looked upon as made up of two elements, Zn | $H_2 SO_4$ (*i.e.*, a Smee), and the element H_2 $Cu SO_4$, so that D = S + H_2 | $Cu SO_4$. Similarly in a Grove G = S + H_2 | HNO_3.

F. EXNER—NATURE OF GALVANIC POLARISATION.

(*Annalen der Physik und Chemie, B. XII., H.* 2, *No.* 2, 1881, *pp.* 280-290.)

This paper is a reply to certain objections raised by Beetz. The author's experiments had led him to the conclusion that the polarisation current was caused by the reunion of the ions separated at either electrode. He opposes the view that any metal, if it is covered with a film of a certain gas, has a definite and constant value of polarisation. In the case of platinum coated with hydrogen, for instance, the value depends essentially on the way in which the hydrogen is employed in the voltameter; and different values will be obtained for the electro-motive force of polarisation according as the second electrode is copper in copper sulphate solution, or silver in silver nitrate. In both cases the hydrogen reduces the metal, but the actions are not thermically equivalent. The author, therefore, considers it valueless to determine the polarisation of one electrode without taking account of the other. He quotes from a paper read before the Vienna Academy:—"The foregoing suffices also to characterise the methods so often employed for the determination of the polarisation in only one gas, *e.g.*, platinum in hydrogen, in this, that the oxygen is employed for the oxidation of the second electrode; but it is by no means

indifferent what metal is oxidised." Beetz throws doubts on this law, and says that Exner has not proved it by experiment, while his own experiments point to the contrary. The author maintains that the former's method was wrong, not the law. The principle of Beetz's method was shortly the following:—The primary current passed through a voltameter consisting of two vessels communicating by a syphon, each holding one electrode, A and B. The vessel containing A was also connected by a syphon with a third, Z, in which was an amalgamated zinc plate in sulphate of zinc. To measure the polarisation of plate A, it is put to earth, and the zinc plate in Z is joined to the electrometer. A was always a platinum plate in $H_2 SO_4$, while B was varied, viz., Pt in $H_2 SO_4$, Zn in Zn SO_4, Cu in Cu SO_4, and Ag in Ag NO_3. The values obtained were always the same, and Beetz concluded that the polarisation of a platinum plate is always the same whatever the other electrode may be.

The author explains that really in these experiments the electrode opposed to the plate was always the same, viz., the zinc plates in the vessel Z. For suppose three communicating cells, Z, A, B, of which Z is connected to the electrometer and A to earth, then the deflection will remain constant, whatever B may be. What Beetz really measured was the electro-motive force of a Smee's cell. The author had also advanced the opinion that the positive pole of a cell, so long as it does not undergo chemical change, acts only as a conductor; that thus in a Smee's cell it is immaterial if the positive pole be platinum, copper, or silver, always provided that they are free from chemical action. Beetz refutes this on the contact theory. He found that in $H_2 SO_4$ the combination Zn — Pt = 0·72, Zn — Cu = 0·46, Zn — Ag = 0·51: the author, on the contrary, found Zn — Pt = 0·73, Zn — Cu = 0·73. He explains the difference by the fact that he employed water freed from oxygen, which Beetz did not do. The oxygen dissolved in the water oxidizes the Cu or Ag and produces a current opposed to that caused by the oxidation of the zinc. Beetz experimented with sodium amalgam, and found Na — Pt = 1·33, Na — Ag = 1·22, Na — Cu = 1·14, Na — Zn = 3·68. The difference in the case of zinc can be explained. The free oxygen no longer plays any part, since the zinc decomposes the water and sets free hydrogen. But this decomposition and the simultaneous oxidation of the zinc sets up a current opposed to that from the Na pole. It is the difference of the two which is measured. It is equal to Na | Pt — Zn | Pt, or 1·33 — 0·73 = 0·60, which agrees closely with the figure obtained by Beetz, 0·68. Beetz does not agree that there is any oxidation of the positive pole, in this case the zinc. He employed a sodium-zinc cell, the metals in separate vessels, connected by a syphon, and left the cell in circuit through a small resistance for 17 hours; an amalgamated zinc plate was also left immersed in acidulated water for a similar time. At the end of the time the acid out of the vessel containing the single zinc plate was found to contain a considerable quantity of salt, while in the cell hardly a trace was found. Beetz concludes that "zinc, when it plays the part of the negative metal in the battery, is not attacked by dilute $H_2 SO_4$." This conclusion is incorrect. The zinc is acted upon, and a solution of Zn SO_4 is formed, but the opposite current from the Na pole, which is more powerful (1·33 to 0·73), again reduces the

oxidised zinc, and only the overplus decomposes the water. In a Na — Pt element the current reduces hydrogen, but in a Na — Zn element zinc: the former work decreases the current by 34 calories, the latter by 52; the difference of 18 calories per equivalent corresponds to the electro-motive force of a Smee's cell. A third point is the course of polarisation in a voltameter when the electro-motive force gradually increases from nothing. The author has laid down the law, founded on experiments, that "the force of the polarisation at each instant is equal to the primary force so long as this does not reach a point which would occasion a continuous electrolysis; as soon as this limit is reached, the force of the polarisation remains perfectly constant, in spite of the further increase of the primary current." Beetz says that this had been already stated by himself and by Crova, and is nothing new.

W. BEETZ—NOTE ON THE NATURE OF GALVANIC POLARISATION.

(*Annalen der Physik und Chemie*, B. XII., H. 2., No. 2, 1881, pp. 290–293.)

The author replies to Exner's objections to his method of measuring polarisation, as given in the latter's paper in the same number of the *Annalen*. If two plates, A and B, serve as electrodes, and then A is put to earth, while B is connected to a neutral plate, Z, A and B are always the electrodes. They are polarised in the customary sense of the word, and the author's researches show that B is always equally strongly polarised by the same current and the same electro-motive force, let the polarisation of A be what it may. If Exner seeks the polarisation not between A and B, but between the electrodes A and Z, then that is quite a different matter. Exner says that, according to the author's experiments, a value of 1·01 Daniell's results for the difference of potential between clean platinum and platinum polarised with oxygen. The latter, however, makes a distinction between a platinum plate polarised with oxygen and one covered with a film of that gas in a passive state. The author is not certain that the silver in a Zn | Ag cell is oxidised by the oxygen dissolved in the water, and even if this be the case, Exner's objection will not hold, as the author had taken all precautions to free the water employed in his experiments from all contained gases. As Exner regards the difference of potential Na | Zn as identical with the algebraical sum of the differences Na | Pt and Pt | Zn, so the difference of potential Zn | Ag must be regarded as identical with the sum of the differences Zn | Pt and Pt | Ag, and if the silver by its oxidation produces a current, the chemical transformations and the heat equivalents must be different in the Zn | Ag cell and in the Zn | Pt, and in that case, by Exner's own showing, the negative plate of a cell would be admitted to its proper share.

F. SCHULZE-BERGE—PRODUCTION OF ELECTRICITY BY CONTACT OF METALS AND GASES.

(*Annalen der Physik und Chemie*, B. XII., H. 2, No. 2, 1881, pp. 293–318.)

Volta has shown the production of electricity by contact of two metals, and subsequent researches have shown the same result on contact of metals and

liquids. The author has investigated the remaining phenomena of contact electricity, viz., those occurring between metals and gases. The effects observed in the so-called gas batteries are not conclusive, as it is quite undetermined how far the liquid in such batteries may affect the results. The author's experiments were made with a condenser (Fig. 1) of two circular discs of similar metal, 15 cm. in diameter. The lower plate (*a*) rested on three set screws, and was put to earth through the gas pipes of the house. The upper plata

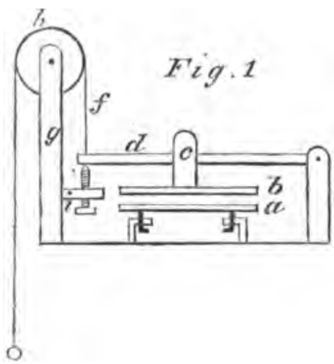
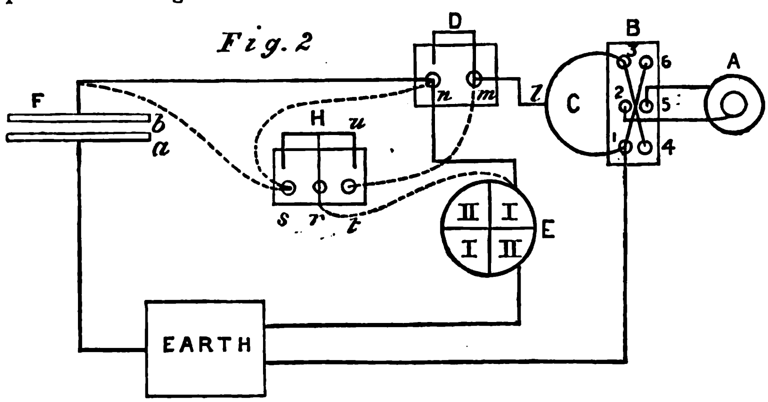


Fig. 1

(*b*) was screwed to a brass piece (*c*), which was insulated from the brass lever (*d*) by a ring of ebonite. One end of the lever (*d*) worked on a hinge in the brass pillar (*e*), and to the other end was attached a wire (*f*) which passed over the pulley (*h*). A set screw (*i*) regulated the distance between the two plates. The upper plate was connected to a Thomson's quadrant electrometer by a wire from *c*. The difference of potential was not measured directly on the electrometer, but both plates were brought to the same potential. Fig. 2 shows a plan of the arrangement.

Fig. 2



A is a Daniell's cell, the poles of which come to the cups (2 and 5) of a mercury commutator (B), by means of which 2 and 1, and 5 and 3, or 2 and 3, and 5 1, can be joined up. C is a rheostat, which contains a constant resistance of 2,200 Siemens' units between 3 and *l*, and a set of resistances from 0 to 10,000

between l and 1. D is a contact breaker with two glass mercury cups, F the condenser, and E the electrometer. If w is the resistance between l and 1, and D the difference of potential of the poles of the Daniell, then at l there will be a potential $s = D \frac{w}{2{,}200 + w}$, and the sign of s will depend on the position of the connections in B.

One advantage gained by this method is that the strengthening effect of one plate of the condenser on the other does not so much affect the result; another is the greater accuracy always obtainable by a null method of measurement. Several sources of error had to be eliminated from the apparatus as first constructed. One arose from the friction of the mercury against the glass cup (n), when the connection (p) was removed from D; the electricity so produced was got rid of by making the cups (m and n) of iron. The points of the connections and the surface of the mercury had also to be kept very clean. The chief source of error, however, lay in the influence of exterior electrified bodies. The approach of the hand or of the clothes of the observer was often sufficient to induce distinct charges on the condenser. To get rid of this influence, the whole apparatus was enclosed in a metal case connected to earth. In spite of all these precautions a slight difference of potential was observed between the condenser plates of polished brass, electro-gilt, which was not constant, but varied between + 0·01 D and — 0·01 D, and was due to inappreciable changes in the surfaces of the plates. After the difference of potential of one plate had been measured and compensated, it was placed under the receiver of an air-pump, and the plate left for some time in a vacuum. It was then always observed to be more positive towards the second plate than before, the greatest value of this change being 0·061 D. It was also found that, even if the one plate were placed under the receiver without exhausting the air, the same effect was produced; so that it could not be determined how far this was due to the vacuum itself or the vapour of the grease used in the air-pump.

In order to try the effect of films of different gases, the upper plate of the condenser was laid on a circular box of brass 1 cm. high, to which it thus formed a lid, and into which the gas was admitted by a pipe through the bottom. In subsequent experiments the gas was made to impinge directly on the plate from a tube leading from the apparatus used for its production. The plate thus coated with a film of gas was then opposed to the lower one which had been in air. When covered with a film of ozone, platinum and brass were both negative to a similar plate in air. With hydrogen, platinum was strongly positive, gold showed no effect, and brass was of different sign in different experiments. Chlorine made platinum appear more negative, while ammonia increased the positive charge of brass. The values for the difference of potential were very unequal, even with similar treatment of the plates, in different observations of the same series. The greatest observed difference was when one plate had a film of hydrogen and the other of air, and was = 0·214 D. In all experiments the difference of potential decreased, and approached the value it had before the gas had begun to act, though it rarely went lower. From this follows that changes of the gaseous films covering metals have considerable

influence on their potential. The film of gas, in fact, acts like a very thin disc of a different metal, and if one of two similar condenser plates is covered with a more strongly positive gas than the other, the former must comport itself towards the second as a more electro-positive metal. The gradual decrease of the difference of potential of two plates, one covered with a film of a gas, and the other with air, is due to the gradual disappearance of the gaseous film, and it remains to be seen whether this occurs by diffusion or by a chemical process. In the former case a difference would arise according as the gas diffused charged with electricity, or in a neutral state.

The author modified his previous arrangement by introducing the commutator (H), by means of which the quadrants (I) could be kept at a constant potential, while the plate was insulated; *p* was removed from *s*, while *r* was connected with *t* through *u*. If the connection (*n*) was then reversed, any alteration of potential of the plate was rendered evident by a deflection of the needle. This happened when the plate was covered only with air, which was probably due to the dust floating in it. The author obtained a curious result if the upper plate was covered with a gas, the lower being covered with air, and the potential of the former being so adjusted that its removal from the second plate produced no change. If the upper plate was insulated for some minutes, and after being brought again near the lower one, was connected to the electrometer quadrants (I), which meantime had been kept at a constant potential, a positive deflection was at first obtained, if the upper plate in consequence of its gaseous film was electro-positive to the lower—negative in the contrary case. But if, then, the upper plate was removed from the lower, the increase of deflection, which was to be expected, was never obtained. In most observations a deflection occurred in the opposite direction, while in some the needle remained at rest. The following table shows some experiments with bromine vapour. Column I. gives the difference of potential of the two clean platinum plates; II., when the upper was covered with the vapour; in III. are the deflections when the upper plate is again connected with the electrometer after five minutes insulation; in IV. the deflections when the upper plate is removed from the lower; while V. is the difference of potential between the two plates at the end of the experiment:—

I.	II.	III.	IV.	V.
0·000 D	— 0·267 D	— 1	+ 1	— 0·223 D
+ 0·007 D	— 0·185 D	— 1·1	+ 1·9	— 0·141 D
0·000 D	— 0·150 D	— 1·5	+ 2·2	— 0·109 D
— 0·005 D	— 0·214 D	— 1·7	+ 1·6	— 0·163 D

Analogous results must follow if the lower plate which is to earth is covered with a film of the gas, as shown in the second table:—

I.	II.	III.	IV.	V.
+ 0·005 D	+ 0·179 D	+ 1·7	— 1	+ 0·134 D
+ 0·009 D	+ 0·221 D	+ 2·7	— 2	+ 0·154 D
— 0·011 D	+ 0·221 D	+ 2·2	— 1·7	+ 0·170 D
— 0·011 D	+ 0·241 D	+ 2·1	— 1·6	+ 0·176 D

The author then proceeds to deal with Exner's paper on the contact of dissimilar metals, an abstract of which was given in the *Jour. Soc. Tel. Eng.*, Vol. IX., No. 34, 1881, p. 462. Exner considers that De la Rive's experiments conclusively show that no electricity is produced by contact of two heterogeneous metals, if they are in a chemically inactive medium. The author shows that there were many sources of error in this experiment, principally due to the way in which the connections were made, the difference of potential of the various points of contact not being taken into account. With reference to the value $\frac{B}{2A}$ set down by Exner for the difference of potential between Zn | Pt, where B is the heat of combustion of Zn, and A the heat value of a Daniell's cell, the author shows that it should be $\frac{B}{A}$. Exner's numbers obtained by experiment agree very well with those calculated from the former value; but they do not agree with those obtained by other experimenters, as the author shows by the following table, in which Zn | Cu is taken = 100 :—

	Exner.	Hankel.	Kohlrausch.	Clifton.
Zn | Pt	171	123	123	—
Cu | Pt	71	23	23	—
Fe | Pt	137	39	—	—
Ag | Pt	16	5	14	—
Zn | Fe	34	84	—	88
Zn | Ag	155	118	109	—
Fe | Cu	66	16	—	12

The author goes on to state that another explanation must be found for Exner's experiment described at the end of the above-mentioned abstract in the Journal; and he describes some experiments of his own. A platinum wire was connected to an electrometer and a strip of silver to earth, the other ends of the two metals being brought into light contact in an empty glass tube, when no effect was observed on the electrometer. A little bromine was then poured into the tube. At first the needle remained at rest, but as soon as the vapour from the bromine had reached the point of contact of the two metals, a strong deflection was observed, which soon reached a maximum. That the presence of the bromine vapour between the two metals was essential was proved by connecting them directly by a binding screw, when there was no deflection. In the case mentioned the deflection was positive, but negative if the platinum was to earth and the silver joined to the electrometer. With the vapour the mean difference of potential from sixteen determinations with

alternately reversed connections was $\pm$ 0·989 D, when the two metals touched in liquid bromine $\pm$ 0·93 D from six readings, and when they were immersed without touching $\pm$ 0·942 D. With chlorine similar results were obtained, and the platinum was always positive to the silver. Chlorine evolved from binoxide of manganese and hydrochloric acid gave a mean value of 0·999 D for the potential; from bichromate of potassium and hydrochloric acid, 1·007 D. These experiments do not explain Exner's results; for if the difference of potential, which Exner observed between the two silver plates, was caused by the action of chlorine on the point of contact of the platinum and the silver, the upper plate should have been positive to the lower one, whereas Exner found it to be negative. Other experiments of Exner's the author considers to be inconclusive, owing to faultiness of method and non-elimination of sources of error.

F. SCHULZE-BERGE—NOTE ON EXNER'S PAPER "ON THE THEORY OF VOLTA'S FUNDAMENTAL EXPERIMENTS."

(*Annalen der Physik und Chemie, B. XII., H. 2, No. 2*, 1881, *pp.* 319-320.)

Exner is of opinion that if the aluminium needle of an electrometer and an insulated copper plate are both put to earth, and afterwards joined by a metallic conductor, a difference of potential is caused in the needle, and consequently a deflection. The author shows that if E is the potential of the earth, which is nil, then when needle and plate are to earth we have the potentials Al | E and Cu | E respectively, and their difference will be Al | Cu. This is just the same difference of potential as will be found if the needle and plate are joined by a metallic conductor. The remaining at rest of the needle is thus no proof against the contact theory. It is also clear that the deflection of a needle which is put to earth, when an E.M.F. is shunted into its circuit, corresponds only to the change produced in its potential, and not to the total value of this latter. Thus, if the potential in the first case is Al | E, then if the current from a Daniell is introduced, the deflection corresponds to the difference

$$\mathrm{Al} \mid \mathrm{Cu} + \mathrm{Cu} \mid \mathrm{F} + \mathrm{F} \mid \mathrm{Zn} + \mathrm{Zn} \mid \mathrm{E} - \mathrm{Al} \mid \mathrm{E},$$

which equals

$$\mathrm{Zn} \mid \mathrm{Cu} + \mathrm{Cu} \mid \mathrm{F} + \mathrm{F} \mid \mathrm{Zn},$$

and not

$$\mathrm{E} \mid \mathrm{Zn} + \mathrm{Zn} \mid \mathrm{F} + \mathrm{F} \mid \mathrm{Cu} + \mathrm{Cu} \mid \mathrm{Al},$$

as Exner assumes. Exner found no appreciable difference of potential between the iron gas-pipes and the lead water-pipes of his laboratory, while the author in his case found a value of 0·00025 Daniell's, which Exner has left out of account in his measurements.

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The Hundredth Ordinary General Meeting of the Society was held on Thursday evening, April 28th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster, S.W. —Mr. Latimer Clark, M. Inst. C.E. (Past President), in the Chair.

The Secretary having read the Minutes of the previous meeting, which were confirmed, and the names of new candidates having been announced,

Mr. Latimer Clark said that it was proposed to resume the discussion upon Mr. Lane Fox Pitt's paper "On the Application of Electricity to Lighting and Heating, and for Domestic and other Purposes."

Capt. M. T. Sale, C.M.G., R.E.: I wish to say one word, Mr. President, with regard to the question of cost. Mr. Lane Fox deprecated any comparison with gas, but, nevertheless, he did, at the end of his paper, make a certain comparison. If I understood him aright, he based his estimate of the cost of a 160 candle-power electric light on that of the coal necessary to produce one horse-power of force, and then compared that with the cost of gas delivered at the spot, assuming 5 cubic feet to be necessary to produce a 16 candle-power light. Now, without venturing to question the relative cheapness of the lights, I would point out that that is an entirely misleading comparison. In the first place, Mr.

Lane Fox takes the price of gas as delivered at the spot. That is more than double that of the coal required for its production. Then, again, he has taken 16 candle-power as requiring 5 cubic feet of gas: this is not fair on the gas. He ought to have taken 100 candle-power as requiring 23 cubic feet of gas—a very different proportion. That is the rate taken by Mr. Alexander Siemens in his paper, and is much fairer. Then, again, he is still more erroneous when, as I understand him, he supposes that the price of coal required to produce one horse-power would be all that is required to produce this 160 candle-power electric light: that is of course entirely incorrect, as the actual cost of the coal consumed would be but one item only, and that by no means the largest, of the expenses of production. I should say that the total cost would be at least four times that of the coal burnt. I mention this to elicit some further explanation, and in order that Mr. Lane Fox's estimate of cost, which appears to me so misleading, should not pass quite unchallenged.

Mr. J. N. SHOOLBRED remarked that the paper was one that dealt rather with the future of electric lighting on a large wholesale extended scale (to which the question was rapidly approaching), than with the present experimental stage, out of which it was successfully emerging. A large number of very important questions, which would have before long to be faced, were raised by the paper, and much credit and many thanks were due to Mr. Lane Fox Pitt for doing so now. Larger generating machines to meet the greatly increased demand; a very much greater number of lights on each circuit than at present in use; the best arrangement of these circuits and sub-circuits in their different ramifications; the capacity and length of the conducting wires; and the regulation of the electrical pressure in the mains to suit the different lamps, and the various conditions under which actual illumination took place—these and other questions, all of great moment and importance, were raised by the paper. They all imperatively require, and must, indeed, each receive its solution, if illumination by electricity is to make the progress which seems now opening to it. But though many thoughtful minds were at

work at them, time and patient research were required for their satisfactory solution.

Of all these questions, that which at the present moment seemed to be the most pressing concerned the conducting cables. All connected with the establishment of electric lighting knew how serious an item of cost the present system, of single-light circuits especially, was becoming, with the complete circuit of separate cable to each. If common returns and earth circuits (such as gas and water pipes) could be used successfully, a great saving in cost would be effected. It was understood that expedients of this nature had lately been made use of with success; and he considered that, if some of the large manufacturers of electrical apparatus which had done so, such as Messrs. Siemens, the British Electric Light Company, and the Brush Anglo-American Electric Light Company, would narrate their experiences on this head, it would prove of value towards an advance in the question. Another point of great importance raised by the paper, and an ingenious solution suggested, was the regulation of the current, so as to preserve an uniform pressure, or electro-motive force, in the mains and at the various lights, under the ever varying conditions arising during the hours of illumination. This regulation was a comparatively easy matter with the incandescent vacuum lights treated of by Mr. Lane Fox Pitt, which will work with almost any machine. But with the larger arc-lights (which, in the speaker's opinion, would not be superseded for some time at least, on account of their considerable economy in the expenditure of mechanical energy required over the incandescent lights) the matter was much more difficult. At present, almost each arc-light required a particular construction of machine. Continuous and alternating current lights, Siemens, Gramme, Lontin, Brush, Werdermann lights, and Jablochkoff candles, and many others, all were in use, each varying in its resistance to the current, and requiring it to have a different degree of tension. At present, in the small installations now being made, each form of burner received its due consideration by having its own particular machine and circuit, as best suited to it. But in the future, say, in the illumination of a large town or district, where the generating

machines would be at some distance, to attempt to lay a series of cables to suit each light, according as it might happen to be asked for, would simply become an intolerable burden of unnecessary expense.

One particular form of machine, therefore, needed to be devised, producing a common current, having properties suited to large lights and to small; and all the burners or lamps, of whatever intensity, must be made to work with this current: all failing to do so would, of course, become useless and obsolete. That this condition will be arrived at is certain; but whether to-morrow, or in one or two years' time, remains in doubt. "Necessity is the mother of invention," and this may, and probably will, bring about the desired solution.

Another subject treated of in the paper, and which in the future will be of much moment, is the suitable subdivision of the electric current in the various ramifications which domestic illumination necessitates. The subject is one about which many misstatements are constantly being made: an increase in the number of lights in series in a single circuit being confounded with the real subdivision due to two or more parallel circuits, or branches, being formed out of the single or trunk circuit. Each day shows that the loss entailed by subdivision, or by multiplication of the light centres, is very much less than was at first supposed. Indeed, the increased area illuminated by means of an augmented number of light centres of moderate intensity, over that lit by a few centres of considerable intensity, generally more than compensates for any theoretical loss in amount of light produced.

As the question of the amount of coal consumed in steam-engines is mentioned in the paper, and has occasioned a good deal of interest, it may perhaps be well to remind the Society that probably the two most valuable collections of information on this head, as to actual performance, are contained, the one in Mr. F. J. Bramwell's paper "On the Economy of Fuel in Steam Navigation, &c," read before the Institution of Mechanical Engineers;*

* See *Proceedings of Institution of Mechanical Engineers*, Liverpool Meeting, 1872.

and treating of the performance of nineteen large ocean steamships on long voyages, all fitted with engines on the compound principle. The other in the Report to the Royal Agricultural Society of the trials of the portable engines at the Cardiff meeting in 1872, presented by the judges, Mr. F. I. Bramwell and Mr. W. Menelaus.* Here twelve of the best known makers competed, all with engines of the portable type, and using high pressure steam only. This class of engine has been so far much used for electric lighting purposes, on account of size, handiness, and of the very sensitive governing gear with which it is almost always fitted, and is therefore especially suited for use with electric light machines. With the compound engines giving off from each 800 to 2,500 indicated horse-power, the average consumption of coal was found to be 1·70 lbs. per indicated horse-power per hour; while with the agricultural portable engines, all of them of 8 horse-power nominal, the average consumption was 4·02 lbs. per indicated horse-power per hour, and this the result of picked experiments at a show-yard, and not of actual everyday work (where 6 lbs. would be nearer the average). This shows clearly the advantage of working on a large scale, and with all the most recent improvements of compound action, and of using condensation—economies which therefore await electric lighting when it arrives at the degree of wholesale production contemplated by Mr. Lane Fox Pitt, in his paper.

Mr. ALEXANDER SIEMENS: I had not the pleasure of being present when Mr. Fox Pitt gave his paper, and I may perhaps misunderstand it to a certain extent, but, as far as I can see from a hasty glance, I disagree with nearly every word of it.

Mr. Fox Pitt commences by saying that "the subject which I am about to enter upon this evening has been the cause of considerable difference of opinion among practical minds, and, I am sorry to say, also of considerable antagonism."

Well, I think that is just the thing we want. We do not want a monopoly for one system of lighting, as being the only one, but

* See *Proceedings of Royal Agricultural Society*, Cardiff Meeting, Vol. IX., 1873.

we want a solid competition, so that everybody is kept to the mark; and therefore I think it is a very good thing that there are four or five different systems of lighting which have to urge each other on.

Then the title of the paper is "Lighting and Heating," but I find no reference to heating at all, except at the end, which says, "From this basis I have calculated that power would not cost so much as one farthing an hour per horse-power, and that, therefore, even for heating purposes, electricity would compare favourably with gas." I do not understand that.

His explanation of the theory of electricity and its diffusion is more mysterious than ever. The usual theory of electricity being a form of motion, which can be calculated out mathematically, and by which all the laws of electricity can be deducted, is very clear, and accounts for all the facts.

I do not want to go through all the points, but with regard to what Mr. Shoolbred said just now, that it was very desirable to put some facts before the meeting about the supply of electric lights, I think that Messrs. Siemens would be prepared to undertake lighting a large district, and distribute the electric light to houses—to light the streets with arc lamps, and the houses with incandescent lights of some kind, the Joel, Fox, Swan, or any other lamp. I am sure that a detailed plan could be at once prepared if Messrs. Siemens received an order for lighting a district; but it would be of little value to lay before the meeting a plan for lighting an imaginary district. The station which Messrs. Siemens have established in the City we intended to connect with the different shops in the streets, and let the people have lamps which they could turn on and off as they please. There would have been no difficulty in making the arrangements: it is only the physical impossibility of putting more machines into the present warehouse which prevented the execution of the plan. Any electrician can make the subdivision of the light, either by connecting in parallel arc or in series with incandescent or arc lamps, such arc lamps being regulated on the differential principle.

In answer to Mr. Shoolbred, there is no difficulty whatever in using the earth for a return wire, provided that, as in the case of

working telegraphs, great care is taken that there is really a good earth connection at the machine and at the lamp, and that not too great a resistance is put in the circuit. With these precautions the earth can be used very well, and has in many cases been used.

Mr. Shoolbred thinks that opinions have changed in respect to the loss of light by subdivision, by putting several arc lights into continuous current. I will take an example from the Brush system. A 16-light machine, when giving 16 single lights of nominally 2,000 candle-power each, produces altogether an illumination equal to 32,000 candles. When, however, the coils are connected up and put into one light, the power given is equal to nominally 50,000 candles. These figures show that the proportion will be 25·16 as representing the increase of light when the total current is used in one lamp, compared with when it is divided.

It is generally believed (though I do not know whether the opinion has been expressed during the discussion) that one system —for instance, the Siemens system—is only suitable for single light machines, and other systems better suitable for producing several lights in one circuit; but that is not at all the case. Machines can be constructed according to any system, whether the Siemens, Brush, Gramme, or any other, to work a series of lamps in one circuit, or a single lamp: it is a mere matter of arrangement, which has nothing to do with the particular system or the principle on which the machine works, but is simply a question of arranging the wire on the machine so as to give more electro-motive force.

I should like Mr. Fox Pitt, if he does not consider it too much trouble, to give us an explanation of the table he has presented, because I am unable to understand it at all.

Then he says, near the end of the paper, "It has often been argued that electricity cannot be stored like gas; but it should be remembered that electricity is not a fuel like gas, and that energy can be stored potentially in many practical ways, and that potential energy can be readily converted at any time into a kinetic form."

I think he might have said it a little less scientifically; he might have said, "The steam-engine which is to drive the

electrical machines can be kept with banked fires;" and I may mention that in that manner the electric light apparatus at the British Museum has been worked during the last two winters. The fire was made, and a certain amount of steam got up in the morning, then the fires were banked, so that when fogs came on suddenly it was possible to raise steam in about ten minutes: thus the light was always ready when wanted, and that is the way in which the objections could be met that electricity cannot be stored.

Mr. H. C. DONOVAN: May I be allowed to ask Mr. Lane Fox a question. I have read several descriptions of Mr. Edison's vacuum light in different scientific papers, and I was present when Mr. Swan read his paper before this Society on his light, and I am rather anxious to know (if the question be in order) in what respect the light of Mr. Fox Pitt differs from that of Mr. Swan. As he has christened his light the Lane Fox light, and Mr. Swan has christened his the Swan light, I am anxious to know in what way one differs from the other.

Mr. C. F. VARLEY, F.R.S., Mem. Inst. C.E.: I was not here when the paper was read, but, in looking over a printed copy of it, I have met with one or two remarks on the part of Mr. Lane Fox Pitt which I think require a little notice from some member of the society. Mr. Fox Pitt describes his view of electricity as that of an elastic fluid, capable of being expanded and diffused *ad libitum*. I will quote his own words. He says: " . . . I conceive it as an extraordinarily attenuated form of matter, capable of diffusing and expanding itself in certain bodies to an indefinite extent—a fluid capable of being *compressed* by the application of force." Now the late Dr. Michael Faraday examined this question very thoroughly, and his experiment cannot be too often kept before our eyes, in order that we may see how thoroughly incompressible electricity is. Faraday's experiment, which I will sketch on the board, was carried out at a time when electrometers of great sensibility did not exist. His experiment consisted of an insulated metal vessel (on a glass stand) into which he placed an insulated bass ball suspended by means of a silk cord. The metal vessel was connected to a gold leaf electrometer. On

charging the ball with positive or negative electricity, and suspending it inside the jar, without contact therewith, the inductive action of that ball upon the vessel caused the gold leaves to separate. If the ball were charged with positive electricity, the gold leaves opened with positive electricity. If, when that was the case, the vessel were discharged, and the ball removed without being allowed to touch the vessel, the gold leaves opened with negative electricity. But the important point of this experiment was that, when the ball was lowered so as to touch the vessel, the whole of the electricity from the ball escaped to the outer vessel, and on removing the ball and testing it, it was found to be entirely free from electricity. I have repeated this experiment with an electrometer capable of showing a potential of much less than one volt, and I have charged the vessel and the ball up to exactly 50,000 elements of Daniell's battery; therefore, had the electricity been capable of being compressed one in fifty thousand, when that ball was removed, after first touching the inside of the vessel, there would have been electricity enough upon it to have shown itself upon the electrometer. That experiment shows clearly that electricity, whatever it be,—whether it be a fluid, or whether it be a form of motion—is incompressible within these limits. With regard to the nature of electricity, I am not prepared to agree with Maxwell that it is a mode or form of motion; but, treating it as a fluid, it is less compressible than water. Some time ago a rather warm discussion took place in the *Engineer* owing to several telegraph engineers of note describing electricity as a form of force. Now one thing we are certain about, and that is, that electricity is a vehicle of force, just as the water in the hydraulic press is not a mode of force or motion, but simply a vehicle of force; and, so far as I can see, there is not in the whole of our experience the slightest indication that we can add to or take from the quantity of electricity in any body. We cannot compress an atom of electricity into a body, and we cannot abstract from that body any of its electricity. There is no such thing yet known as latent electricity, but latent force and latent heat are found everywhere.

With regard to the table, I must confess, like Mr. A. Siemens,

that I do not understand it. In his paper, Mr. Fox Pitt says: "I should mention that the resistance of a carbon filament is about 45 per cent. less while in a state of incandescence than when cold." Now I really should be very glad if Mr. Fox Pitt would give us some information upon that subject, because the table seems to indicate the very reverse, and, if I understand the table aright (but I do not think I do), I would like to know how, in a matter of this kind, involving much difficulty, he can pretend to be accurate to the *five* places of figures he has given in his table, for I know from experience that, even in the measurement of the resistance of a coil of wire, it is extremely difficult to be certain to 1 in 100,000.

Mr. R. E. CROMPTON: Mr. President,—As most of you are aware, I am labouring under the great disadvantage of having spoken on the subject at the commencement of this discussion last meeting, and, my first remarks having already been taken down, I run a great risk of repeating myself unless I cut what I have now to add down to a few words.

I have had some conversation with Mr. Lane Fox Pitt since I made my previous remarks, and, as I think he partly misunderstood something I said at the time, I will now explain, and consequently save him the trouble of replying to them.

I found fault with his system of distribution, because I understood that the practical limit of resistance within which it was possible to construct single lamps was not great. He now tells me that there is no practical limit to the resistance of the lamps—that he makes them as easily to 200, 300, or 500 ohms as of 60 or 80 ohms. Of course, if that be really so, it alters the question, because that in some measure answers me as regards the want of economy I alleged against the parallel system of working these lamps. Because it is evident to every one that it matters very little whether one lamp of 500 ohms or five lamps of 100 ohms each are put in series; it all comes to the same effect as regards bridging from the electrical main conductor to the return wire or earth, although it may be material to the economy and distribution of the light; and so, having explained this, I will relieve Mr. Fox Pitt from the necessity of answering that part of the question.

Another point, which I ought to have mentioned in my previous remarks, is with reference to the cost of engine-power. Mr. Lane Fox Pitt dwelt very much on the low figures that are now obtained in coal consumption—that one horse-power could be got for about 1¼ lbs. coal consumption. But he must remember that these are very low figures. Anything below 2¼ lbs. of coal per horse-power can only be obtained by means of very great complication of the valve gear of the engines, which must very much increase the first cost, and the liability to break down and interruption; and I think that, when you consider the very great necessity there will be of subdividing the engine-power, even if there be a large central station, instead of having one big engine, (the failure of any small detail of which would stop the whole lighting of the district)—granting this necessity of subdivision, it is very necessary to have each of these smaller engines exceedingly simple in their details, to get which simplicity and trustworthiness it will be necessary to sacrifice a little of the economy in the coal bill. I think we shall get as low as 2¼ lbs. of coal per horse-power—in fact, I know there are firms now prepared to supply engines of 16 horse-power nominal and upwards, which they will guarantee to work with 2¼ lbs. per horse-power. Every tenth of a pound below that figure is very difficult to obtain, and I do not think we shall get as low as 1·5 lbs. But even if we do get to that figure, referring to that part of Mr. Fox Pitt's paper in which he proposes to use the current for heating purposes, it would rather surprise you to see the small amount of heat you would thus get out of 1 lb. of coal. Even with such a perfect steam-engine, and the most perfect dynamo-electric machine we now have, at the end of the circuit you would only get about nine per cent. of the total heat units contained in that pound of fuel, whereas 62 per cent. of the total heat units has been frequently obtained by direct use of the coal under a suitable boiler. With a less perfect engine, 4 per cent. would be all that you could count on as available for heating by electricity. The table beneath shows the exact figures.

Table showing heat units in a pound of coal, and percentage utilised in steam-engines using 1·5 and 3 lbs. per hour, also units available for heating by electricity.

	Total heat units in 1 lb. of coal, mean of 97 kinds.—*Vide* Govt. experiments under Playfair, De la Beche, and others.	Units available in evaporation of water in Cornish and other boilers.—Govt. experiments.	Units in horse-power, generated by Perkins Engine Co. and others, at 1·5 lbs. coal per actual horse-power per hour.	Units in horse-power of a good economical engine of commercial type, using 3 lbs. coal per horse-power actual per hour.	Units available in a heating coil forming part of external circuit of a good dynamo machine, utilising 70 % of the horse-power in its external circuit, using 1·5 lb. engine.	Do. do. using 3 lb. engine.
Total Heat Units.	13,006	8,083	1,715	857	1,200	600
Percentage utilised.	100	62	13·2	6·6	9·25	4·6

These small percentages point out that, so long as we have to use the present steam-engine as the source of energy, we have little to hope for in economic distribution of heating, cooking, etc., by means of the electric current.

That need be no discouragement to us; for the applications we get on all sides, from people who want light alone, are so numerous, that we can afford to let the question of distributed heating rest for a year or two.

Professor J. PERRY: Some remarks have been made about the numbers in this wall-sheet. It may be well just to intimate that, when the printed copy was in my hands a few minutes ago, I took the liberty of finding whether in one of the columns, say, the column for the 15-candle light, by squaring the electro-motive force and dividing it by the resistance, you could get a constant or nearly constant number. One would imagine, of course, that if light were the only energy given out by the carbon, then $\frac{e^2}{r}$ ought to remain a constant, since the light for all these numbers is the same; but, as the carbon also gives out heat, one would hardly expect to get a constant, as $\frac{e^2}{r}$ represents the total energy given out by the carbon. I find that in several cases, taking the numbers in the first column, $\frac{e^2}{r}$ is exactly 75, and I am

inclined to think that, if any column is taken, the same result will be arrived at—a constant number for the quotient in question; and from this I infer that these figures have not been deduced from experiment at all, but have been calculated. Perhaps one quotient has been deduced from one experiment, and all the numbers calculated. If I am correct, then, I think that the fact might have been mentioned in the paper; at the same time the paper does not say that they are experimental numbers, whatever inferences we may have drawn. This would also dispose of Mr. C. F. Varley's wonder at there being six significant figures in some of the numbers, because they merely indicate unnecessary calculation, whereas he thought that they indicated extraordinarily delicate apparatus.

It is with great regret that I must decline to add anything to the general discussion of this valuable paper.

Mr. C. F. Heinrichs: Several gentlemen asked at our last meeting, and another gentleman has done so this evening, what is the difference between the Lane Fox, the Swan, and the Edison lamp. I shall not fall into the same error. You must not expect to have anything new,—"there is nothing new under the sun," that we know,—but the main thing is: Is the invention perfected? Has Mr. Lane Fox Pitt got a new method of producing and maintaining a vacuum? Are the carbons durable? He says "yes;" and I think that is a sufficient novelty for us to discuss, and, if not disproved, would satisfy us that Mr. Lane Fox's lamps are perfectly new. I do not agree with Mr. A. Siemens in saying that you can make a machine give a higher electro-motive force by merely lengthening the wire on the armature.

Mr. Alexander Siemens: I did not say so.

Mr. C. F. Heinrichs: You cannot put two or three lamps in any machine; the systems are different. A Siemens machine cannot be made to give 24, 25, or 30 lights as the Brush machine can, nor can the Brush system be made to give a small light as the Siemens can. The main difference is in the construction of the armature, a point of detail too technical to go into now.

Mr. Alexander Siemens: What I wished to say, and still maintain, is, that we can arrange our machines to give as much electro-motive force as we desire; in fact, we have machines now

which give 12 lights in circuit with continuous currents. The machines are constructed in the usual manner—an iron cylinder wound with wire moves between magnets.

Mr. J. A. BETTS then made some remarks about the use of the words "multiple arc," etc., suggesting that, when lamps were put in series, the arrangement should be called "continuous circuit;" when each lamp had a distinct circuit, all, however, being fed by a common dynamo machine, it should be called "divided circuit," and that the words "parallel circuit" should be kept as ordinarily used.

Mr. LATIMER CLARK: We will now take any observations which members may wish to make upon Professors Ayrton and Perry's communications on "A Portable Absolute Galvanometer for Strong Currents," and "A Transmission Dynamometer."

Prof. AYRTON: Regarding the remarks made by Mr. Crompton at the last meeting, that the transmission dynamometer devised by Mr. Perry and myself had been anticipated by a young man working with Mr. Crompton, permit me observe that the exact form of dynamometer referred to by Mr. Crompton, and which he has been good enough since to describe to me, was in reality thought of by Mr. Perry and myself, but rejected by us as being inferior to the one we had the honour of bringing before you. Mr. Crompton's form consists of a loose pulley connected by a spiral spring to a boss feathered on the shaft—that is to say, the boss can move along the shaft, but not round it. If now the shaft be turned and the loose pulley be resisted, the spiral spring which encircles the shaft will be more twisted, and the boss will be pulled towards the loose pulley, and the amount of its motion can of course be easily observed. Now the reason why we rejected this form of dynamometer was, because in such an arrangement there is friction between the boss and the feather on the shaft, resisting the longitudinal motion of the boss; in fact, it possesses all the defects of an ordinary spring-balance when not held vertically, and when, as is well known, the indications are faulty from the friction offered to the motion of the pointer.

In the apparatus which we are having made, and the construction of which was described, there is no such frictional resistance, and hence its indications will probably be more accurate.

Mr. LANE FOX PITT: Mr. Chairman,—At our last meeting I began by saying that this was a subject upon which I thought there was a certain amount of antagonism. I think the remarks of last meeting and of to-night show that I was not very far wrong—there has been a very considerable amount of antagonism. I am not prepared to say that it is wrong that it should be so, but, at the same time, I think that it ought to be sometimes a little more discreet.

Mr. David Chadwick, I think, opened the discussion: he said that, as one of the public, he was not much interested in scientific technicalities, but he would put one or two questions on more general grounds. He asked how much it would cost to light a certain house, but he did not give us many particulars. He said, "if the rental was £100 a year, how much would it cost to light it by electricity; also, another house at £200 a year." That is difficult to say, because it would depend upon other considerations besides the rental. I am reminded of the problem, "Given the price of herrings at two a penny, what would be the price of a loaf of bread?" I do not think it would be possible to answer that. As regards Mr. Chadwick's question, it would depend in the first place upon the locality of the house—chiefly whether there were means at hand to supply electricity from a certain source. If it were near a source of natural motive power, such as the fall of water in a weir, or the force of the tides, or even a windmill, the energy would be exceedingly cheap; and the only other cost to be taken into consideration in such a case would be the interest on the plant, and the necessary attendance. I do look in the immediate future to the possibility of applying the energy from natural sources for lighting, and also, I will venture to say, for heating purposes. We have unlimited sources of energy in nature, and I do not see at all why these should not, by means of scientific appliances, be brought to practical uses through the agency of electricity.

Another question which Mr. Chadwick asked was what was the difference between my light and Edison's light and Swan's light. I do not know if I ought to have begun at the beginning of the whole subject, but I may say that, according to the theory of light, they are all very much the same—they are all three supposed to

be due to the vibration of a luminiferous ether, and so in the light itself there is really no difference. There are, however, certain differences in detail in the construction of the lamps. I am the very last person in the world to depreciate the work of others for my own benefit. I do not think it is profitable or becoming, and I have always thought that Mr. Edison's work on the subject was of the very highest importance. I believe that no man, perhaps, has done more for the benefit, not only of electric lighting, but for the science of electricty itself, than Mr. Edison. Perhaps it has not altogether been owing directly to his own energies that this benefit is due, but, to a great extent also indirectly, to the general exitement which his extraordinary genius has produced, and the manner in which this has led other people to work in the matter; but I do think that there really is the highest credit due to the man who, with the enormous reputation that Edison had two or three years ago, boldly said that he thought electricity would be very soon used for general domestic purposes, and I think the time is not very far off when he will be proved to be right. As to lighting by the incandescent lamp, no man can monopolise that: it has been worked at for over 30, perhaps 40 years, I believe, and I began working at it myself before anything was known of Mr. Edison's discoveries or inventions. In fact, I have patents for England, if I may refer to them, dated some time before Mr. Edison's first, that has of course entitled me to work upon the subject, and to consider that the field is open to me also.

What Mr. Crompton has said to-night takes the sting to a great extent out of his remarks at the last meeting; but I must say that I do not think he was at all kind in his criticisms of my system. He was rather severe at times, and certainly was more kind to Professor Ayrton in offering to buy one of his (Professor Ayrton's) machines, although he said, at the same time, that there was a lad in his office who, two years ago, invented the same thing, or very much the same; only I think he said the child's was much better. Then Mr. Crompton said he did not want to tread upon any one's toes, as he had been asked to do; but I think, nevertheless, that he proceeded to do so to the best of his ability. He said that there would be very considerable waste (or loss, I

think, was the expression he used) from using earth returns—that the current would go to earth and would be to an extent lost.

Mr. Crompton: No, no.

Mr. Fox Pitt, continuing: I think you will remember saying that there would be loss by the use of earth returns. I may have been mistaken, but I would explain what I meant by "earth," which will also serve as a reply to what has been said by another speaker. I would resort to uninsulated conductors in connection with the earth, so that their resistance would be reduced by the conductivity of conducting matter in their vicinity, or in contact with them.

Mr. Crompton altogether found fault with the method of what I will now venture to call a "divided" system. He said that he had tried experiments himself with the dynamo machine, and had obtained certain results, viz., that 18 lamps could be put together in series, but only 10 in multiple arc. I can only say that, under such conditions, seeing that the resistances would be as 1 : 180, a dynamo machine that will work at all is a remarkable thing. However, the question of whether the light can be economically employed on multiple arc entirely depends upon whether the machine is suitable for the purpose. I know perfectly well that it is highly desirable to keep the greater part of the resistance to be overcome outside the machine, in order to get economical results, but I cannot conceive very much difference in economy between such work in "parallel circuit" and in "series," provided the resistances are properly adjusted. The energy developed by a current is exactly proportional to the resistance; and, if we have a certain difference of potential between the two poles of a lamp, that difference of potential would produce or develop a certain amount of energy, and it really does not matter how the electricity arrives at the lamp, whether it is from another lamp, or whether it is directly from the supplying machine. There certainly would be an enormous advantage in using a system of multiple arc in practice over a system of lamps in a continuous circuit or series, because, of course, if one lamp in a series goes out, the whole in the series go out; and then it would not be practicable to lay down so many wires as would be necessary on an extensive scale.

I have to thank Mr. Shoolbred for his remarks. I certainly did intend my paper more as a suggestive or foreshadowing paper —one, as it were, laying forth ideas which might be realised in the future, and putting down the lines on which we were to work, and by which we might arrive soon, I hope, at some extended application of electricity.

Mr. Mitchell asked one or two questions about the belt slipping. He said he had always noticed, at every exhibition of electric lighting, that there was something the matter with the belt of the engine, and asked how it could be overcome. There are three ways in which that could be avoided. First, by using belts sufficiently tight; secondly, by making the belts sufficiently sticky; and thirdly, and the best way, I think, would be to do away with belts altogether. I believe I am right in saying that the Brush lamps in the City are now worked by direct-acting engines, and certainly that is a practical demonstration in answer to the question.

I forgot to mention about using machines or electrical generators themselves in parallel circuit. Mr. Crompton said that he did not think it would be possible. I have been told, and believe it to be true, that at the present time Mr. Edison is using at Menlo Park seven machines in multiple arc, and is working a system of 480 lamps with them.

Mr. Gumpel and others criticised me very severely on the question of economy, and also on the point as to what extent we could rely upon figures given us up to the present day. Mr. Gumpel altogether disagreed with my statement that 1½ lbs. of coal would generate one horse-power per hour in a steam-engine. I think he admitted that 1·6 was a practical figure, and had been practically accomplished, but he would not give in to 1·5. But notwithstanding that, I will adhere to 1·5, and believe it is a practical figure. I know that there is a firm in Greenock, Robert Steel & Co., who advertise that they are prepared to supply engines above 1,000 horse-power at a guaranteed consumption of 1½ lbs. of coal per horse-power per hour. There are many large steamers in which the consumption of fuel is even below that mark. I believe that, in the ships built on the Clyde for the

Chinese Government, the consumption of coal was very little more than 1·3 lbs. per hour per horse-power indicated; and I would remind my critics that, after all, this only reaches about 15 per cent. of the total energy of combustion of coal which is turned into useful motive power, and I cannot really see why they should so complain at my taking practical figures of the present day when I am speaking of the possibilities of the future. I think that it is exceedingly probable that the figures of 1·5 lbs. of coal may be very much reduced; in fact, I do not see why, by using high pressure steam and good engines on a large scale, the consumption should not be enormously reduced below present standards.

I must run over the other points rather quickly, for I fear I have taken up your time too much already. Mr. Shoolbred, in his remarks, quite comprehended what I had intended to convey by my suggestions, viz., that I was not dealing with practical results, because, when I spoke of large centres supplying thousands of lamps, I could not possibly be speaking of what had already been done; and I think Mr. Shoolbred showed that he thoroughly appreciated the advantage of a discussion of this sort, which would probably assist further development, and could do no harm. In any difficult matter, the more fully and freely it is discussed, the more familiar it becomes, and, after all, familiarity is what is needed. The subject is, to mere scientific men, more or less simple, but what we want is to direct a great amount of intelligent labour to what will probably be a very profitable source of employment; for I believe the future applications of electricity will open up an entirely new industry, and that we shall have very soon a vast amount of at present unemployed labour earning an immense amount of money, at the same time advancing science, and the general welfare of the nation and the world.

Mr. Shoolbred spoke of the electric arc as being one of the most important branches of electric lighting on a large scale, but I must say that I never intended the system I have described to be used for the electric arc light as well as the incandescent light. I must say that I think there is a possibility of arc lights disappearing altogether, for I do not see why we should not ultimately get carbon to resist as high a temperature permanently as it does

temporarily in the pencils of the electric arc. There is nothing really against this; and, besides, it is not necessary that the temperature should be so high in an electric lamp. In estimating the cost of the arc light, we have to take into account a great number of items besides the one item, the cost of the power. In the first place, there is the cost of the carbon pencils, then the cost of the lamp or regulator and its wear and tear, and the skilled labour required to adjust and put the pencils in their place, and then there is the trouble of connections; which are altogether avoided in the incandescent lights, where we have merely to connect the lamps up once for all, and if it is properly made I believe it will endure for an indefinite length of time and at any time give the required light. There is no doubt that at present the energy absorbed by incandescent lamps to produce a unit of light is very much greater than the energy absorbed to produce the same unit by arc lighting, but, every consideration taken into account, I believe that you could very possibly compete with it in point of actual price.

With reference to earth returns, I meant merely connections of existing systems to earth, so to speak, such as water-pipes or gas-pipes where they exist; and the resistance of gas and water-pipes where you are dealing with small currents, such as those passing through an incandescent lamp (generally something very much less than the unit or weber), would be so small that it need scarcely be taken into account.

Capt. Sale made some similar remarks about the cost. He said that I had confined my observations entirely to the cost of coal. I must beg to differ from him. I have taken the cost of coal as being $\frac{1}{16}$ of a penny per horse-power on a large scale, and I have estimated four times that amount as the total cost, and that would bring it down to a farthing; and so I did exactly what he recommended should be done.

I quite agree with the last speaker (Mr. J. A. Betts) that it would be most desirable to have an uniform nomenclature for the different terms used. I would also suggest that "incandescent" lighting is a slightly inappropriate term to use for lighting by means of the incandescence of a continuous conductor. Of course

all practical artificial light is the result of incandescence of certain particles. I have suggested myself the term of "bridge" lighting, in contradistinction to the "arc."

I had forgotten Mr. A. Siemens. He does not at all agree with me, but he did not, I think, really explain very fully on what points he disagreed. He said that he did not understand the table. I have also overlooked Professor Perry's criticism, that the table was the result of calculation. Certainly it is. There were one or two experiments which I made, and for the sake of convenience I deduced the figures so as to give an idea of what the resistance of a lamp should be to suit any electro-motive force, and to show that, by using a somewhat higher degree of electro-motive force in the system, the resistance of the lamps would have to be increased in a much greater proportion. Mr. C. F. Varley, too, I think, mistook the whole table as referring to the same lamp. It represents the same light, the same candle-power, but each different row of figures represents a different lamp, as suited to a different electro-motive force. For instance, take the top line in the table: we have a system of lights in which the conductors are charged constantly to 30 volts of electro-motive force. In order to obtain here a 15-candle light having a luminous surface of $\frac{1}{16}$ of a square inch, and absorbing $\frac{1}{10}$ horse-power of energy, the resistance would have to be 11·95 ohms. The appearance of the lamp in every one case would be very much the same; the difference would only be that the resistance of the conductors for high electro-motive forces would be much greater. I have merely compiled the table to give some people, who may not be very well up in the matter, a general idea of how any electro-motive force could be used, and how by only slightly, as it were, increasing the electro-motive force, the resistance of the lamps must be enormously increased to correspond; and so, by using very high electro-motive force, very great economy would be obtained—economy, that is to say, in point of the size of the conducting mains.

Objections were raised to the form of regulator. I think that Mr. Crompton and Mr. Gumpel both said that the form of regulator for moving a throttle-valve would not be practicable at the present day. On a reference to this diagram (which represents a

machine that would give any amount of power), it will be seen that the arrangement involves a vibrating hammer, similar to the hammer on an induction coil: this oscillating hammer, or armature, working on a ratchet, moves a wheel round, and, though the motion is slow, an enormous power could be obtained which would overcome the objection raised to the plan. This (Diagram VI.) was merely diagramatic or typical. It is quite immaterial how the electro-motive force is controlled. Mr. Crompton was not quite right when he said that in the Brush system they had a perfect regulator. I do not mean that the regulator is not very perfect, I daresay it is, but I mean that it is for a very different purpose, its object being to ensure a variable electro-motive force from the machine in proportion to the number of lamps in operation, so that in the circuit there is always the same amount of current. That is not my object, which is to regulate the pressure so that it is constant during great variations of current.

I am very much obliged to Mr. Varley for his explanation as to the non-compressibility of electricity. That is not a matter of very much importance, if we are agreed upon the main subject, that it is to be looked upon rather as a medium for transmitting power than anything else. But I cannot quite understand how, if electricity is a fluid and not compressible, a condenser of a given capacity, when charged to a certain tension, will be capable of charging several other condensers of equal capacity, or a condenser of greater capacity, with electricity, to an inferior tension in proportion to the tension of the original, without the electricity expanding. However, that is a point which I do not think of very much consequence. With proper appliances, we can always get a proper return for the energy expended.

Professor AYRTON: In justice to Mr. Crompton, may I be allowed to correct one misconception on the part of Mr. Lane Fox Pitt. The two communications of Professor Perry and myself were quite distinct, although made one after the other. The first, you will remember, was bringing to your notice a portable galvanometer for measuring the strongest currents, and which could be calibrated with a weak current produced by only one Daniell cell. It was this little instrument that Mr. Crompton expressed himself so desirous

of obtaining, and the invention of which was not attributed to an assistant of Mr. Crompton.

The second communication, which was quite distinct, was on a transmission dynamometer, which could be put on the main shaft of any factory, and not a laboratory instrument like the ordinary transmission dynamometer; and I think it quite possible, after the few words I have said this evening, showing the advantage of our form of transmission dynamometer over that employed by Mr. Crompton (and which was devised by his assistant), that he may express himself also equally desirous of obtaining that too.

After a few observations of the Chairman regarding the practical value of the papers contributed by Mr. Lane Fox Pitt and by Professors Ayrton and Perry, a cordial vote of thanks was returned to the authors.

The One Hundred and First Ordinary General Meeting of the Society was held on Thursday evening, May 12th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Professor W. G. ADAMS, F.R.S. (Vice-President), in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of candidates for admission announced and ordered to be suspended.

The following paper was then delivered:—

RADIOPHONY.

By W. H. PREECE, Past President.

A sower went forth to sow, and his seeds, falling on various soils, met with various fates. Some fell on stony ground, and other on fruitful ground; but the seeds of few sowers ever fell on such fruitful ground as those of Graham Bell and Sumner Tainter. The facts brought forth by their researches showed that "sonorousness under the influence of light is a property common to all matter," or, in other words, if matter, either in a state of liquid, solid, or gas, was submitted to the influence of intermittent light, the result was the production of sounds.

Inasmuch as physicists for 200 years had been trying to produce some experiment which should show that bodies moved under the influence of light, and failed; and inasmuch as 60 years ago Bennett concentrated the rays of the sun on very sensitive suspended substances, but obtained no motion at all, and many have used that as an argument against the corpuscular theory of light; so everybody was startled by Bell's announcement.

When Professor Graham Bell announced before this Society that the sound produced by light rays which were broken up and made to fall intermittently on discs of hard material was a property of light, you will remember that Professor Tyndall expressed his belief that the effect was due to heat, and not to light. Dr. Spottiswoode, P.R.S., suggested the same thing in his last presidential address to the Royal Society. Every experimenter since has shown unmistakably that the surmise of Dr. Spottiswoode and the opinion of Dr. Tyndall were quite right.

Many enquirers have been investigating the subject, and it is rather amusing to notice the various titles adopted by them. Graham Bell, in his paper of the 27th August, 1880, adopted the title, "Upon Production and Reproduction of Sound by Light." As recently as 21st April, 1881, in a paper read before the American Society of Science, he discusses the same question under the title, "Upon the Production of Sound by Radiant Energy." M. Mercadier, the head of the technical school of the telegraph administration in Paris, an extremely able experimenter, as well as a very clever physicist, has written several papers from week to week, published in *La Lumière Electrique* (and which have been read before the French Academy), which have invariably been entitled "Notes on Radiophony." A German physicist, Röntgen, on the 8th December last, read a paper on the same subject, which he entitled, "On the Tones which arise from the Intermittent Illumination of a Gas." Dr. Tyndall, on the 13th January, 1881, read a paper before the Royal Society under the title of "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter;" and, on the 10th March last, I read before the Royal Society a paper which I entitled, "On the Conversion of Radiant Energy into Sonorous Vibrations." But, as will be gathered from the title of this evening's paper, I propose in future to speak of the phenomena as phenomena of "Radiophony."

Before going further, it would be as well to make it clear as to what is understood by these various terms. By Radiophony, the term adopted by Mercadier, Bell and Tainter, and myself, I simply mean the production of sounds by radiant energy. The term "sound" is introduced, and it is necessary I should explain why that term has been converted into "sonorous vibrations." I take the term "sound" to express a certain action which takes place in the brain: sound is a sensation. "Sound" is a term applied to an act of consciousness, such as happens at the present moment when you are listening to my voice. Sound alone reaches the brain; outside your ears, between you and me, there is no such thing as sound. When my lips move, a disturbance or motion of the particles of the air is set up, and this motion is sonorous vibration, which is translated by the ear and communicated to the brain as sound.

Sonorous vibrations, then, are mere waves or motions of particles of the air.

By "radiant energy," physicists now speak of the motion of the ether, that highly elastic medium which fills all space. The heat of the sun, the light of the stars, the effects which we call actinism, and all the physical effects that pass between astronomical bodies, are transmitted by this medium, ether, and its movements or vibrations are called radiant energy. Sometimes this action is called "radiation," and it is a very frequent thing to see in papers at the present day the word "radiation" so employed. The latter term is perfectly accurate when the meaning is properly defined, but it should only be applied to the production of an effect, such as that produced by the gas jets now burning above us; but if we call that which produces as well as that which is produced by the same term, we commit a logical defect—that is, we use anomalous terms. Many physicists have found it better to speak of radiation as one thing, viz., that producing, and of radiant energy as another thing, viz., that produced.

So it has now become the practice to speak of radiant energy as expressive of that motion of the ether which, when falling upon the retina of the eye, produces light; which, when falling upon certain bodies, especially upon such things as lampblack, produces heat; and which, when falling upon salts of silver and of various other metals, produces that particular effect called photography. This motion varies only in wave length. The terms used in old text-books, dividing the rays of the sun into luminous, thermic, and actinic rays, have in modern scientific text-books dropped out of existence, and all these effects are simply attributed to the different lengths of the particular waves that agitate the ether.

The great advantage of the term Radiophony is that it is perfectly general. It means the production of sound by the impact of the waves of radiant energy upon different materials. If the sounds are produced by the impact of the wave lengths that fall between the red and violet (which is called the visible part of the spectrum), they produce what is called the photophone. If, on the other hand, these waves of radiant energy are those which fall on

substances and produce heat, the effect is called a thermophone; and it is not at all unlikely (although I do not think that it has been done) that the waves of highest refrangibility (far above the violet) could be utilised in some way to produce sound, and be called an actinophone.

With regard to a definition that is sometimes used, I should like to explain the distinction that is drawn between radiant heat and thermometric heat. *Radiant heat* is wave motion in the ether; *thermometric heat*, wave motion in matter. The greater the swing, the greater the amplitude of motion of the molecules of which matter is composed, the higher is the temperature and the greater the heat present. The difference between radiant heat and thermometric heat, therefore, is that radiant heat is the motion of ether, and thermometric heat is the motion of matter.

The former we cannot measure; the latter we can measure by the aid of a thermometer, and therefore it is called thermometric heat.

What are the facts that we have to deal with?

First of all, let me explain the apparatus that I have before you, and which, by the aid of my indefatigable friend (Mr. Stroh), I hope to show you.

Here is an oxyhydrogen light which, though perhaps not so effective as the electric light, is more amenable to control, does not possess so many faults, and is sufficiently powerful for our purpose.

The oxyhydrogen light falls on a line of perforations near the periphery of a zinc disc which can be rotated. When the light passes through the perforations on the zinc plate, its rays are rendered intermittent and are received upon a lens which gathers them together, or brings them parallel, and they then pass on to a disc inside a small square case. In this case can be placed discs of various materials, and on the side farthest from the light (and at the back of any discs placed in the case) a hole is cut into which is fitted a flexible hearing tube. At present a disc of ebonite is placed inside the case. I am afraid that these sounds will not be heard beyond the tube; I will therefore ask the audience to accept (during the reading of the paper) Professor

Hughes' remarks on the success of the experiments as they are carried out.

Exp. 1.—The zinc disc is now rotating, causing the light to fall intermittently on the lens, and so on to the ebonite in the case. I should say that the sounds follow the rapidity of rotation of the zinc disc.

Professor HUGHES: I can hear a musical whistling sound, quite distinct from the whizzing noise of the zinc disc rotating in the air.

Mr. PREECE (continuing): The question that is raised by this experiment is, seeing that rays of light, or, as I should say, of radiant energy, in falling intermittently upon the ebonite disc, produce sound, to what particular branch of these rays is the sound due? Is it due to waves of higher refrangibility, to waves of lower refrangibility, to what of old was spoken of as light, or to what is more frequently spoken of as radiant heat? Mr. Graham Bell, in his original paper before this Society, attributed it to the light. M. Mercadier, who was the first to take up this subject in Europe, and who worked at it with great energy, proved that they really belonged to the red and ultra-red rays, and that they varied with the substance used as a disc; and, as I have already said, Dr. Tyndall stated in this room that he believed the effect was due to radiant heat. I think we shall in the next experiment be able to show that it is not the effect of light but of radiant heat.

Exp. 2.—Ebonite is almost absolutely opaque: with a powerful light (such as the electric light or the sun) a dull red gleam may be seen through it if it be thin enough, but to all intents and purposes ebonite is opaque to light. Ebonite, however, allows the lower rays of the spectrum to pass through it: the extreme verge of the red, and all the lower rays pass through thin ebonite. Capt. Abney has succeeded in photographing the carbons of an electric light through a dark piece of ebonite, and has, in fact, photographed the spectrum through that material.

Therefore, on holding a thin sheet of ebonite in an intermediate position between the lens and the receiving case, I shut off the light from the latter. But does that cause the sounds to cease?

Prof. HUGHES: No; I can hear the sound as clearly as before.

Mr. PREECE (continuing): That experiment proves that the

effect is not one due to the light, but to the heat rays which pass through the dark ebonite.

Mr. Graham Bell, in his excellent paper, to which I have already referred, has accepted this conclusion; but he has carried the proof a step further, and has shown (as I will also show you presently) that the effect not only varies with the substance upon which the rays fall, but with the *colour* of the substance. Hence the conclusion is clear that the sounds are due to the conversion of radiant energy into thermometric heat. The rays of radiant energy passing from the lamp on to the disc become converted on the disc into thermometric heat, and produce sound.

Then comes the question, How is the sound produced? Is it an effect due to the direct expansion and contraction of the mass upon which the light falls? It is very easy to understand that if light fell on a substance like ebonite, and if that expanded with the heat, and contracted when the heat was removed, and if the periods of expansion and contraction followed each other with sufficient rapidity, we should get an expansion and contraction of the air in contact with the thin disc, which would be transmitted to the tympanum of our ears and there produce sound. The assumption of the expansion and contraction of a mass of material like this involves a question of time. To expand and contract a body—to heat and cool a body—involves time. Although Lord Rayleigh, in a paper that he wrote, showed that it was possible, by the rapid heating and cooling of a body, to produce this rapid expansion and contraction, and although Mr. Graham Bell (in a paper about to appear in the scientific journals) maintains the same position, yet, with all the skill, pains, and care that Mr. Stroh and I have devoted to the matter, in the ordinary instruments, we have failed to obtain any evidence, or at any rate sufficient evidence, of expansion and contraction of these discs to account for the sonorous vibrations which Professor Hughes just told you he heard. Mr. Graham Bell, in his paper (to which I have just referred), does not consider that the experiments I have made are sufficiently adequate to account, even in a negative sense, for this view of the question. I consider it perfectly and absolutely proved that, under the influence of such a light as the oxyhydrogen light,

we cannot produce these sounds in that way; but the solar rays may be more powerful in Washington, where Mr. Graham Bell carried out his experiments. We have used that most sensitive instrument the microphone, attached to the disc and case in every form, to try and prove the existence of the mechanical vibration of the disc, and we have failed. Therefore we may say, unhesitatingly, that the sounds are not due primarily to the expansion and contraction of the disc.

Are the sounds due to molecular pressure? What is molecular pressure? I have here a radiometer devised by Mr. Crookes, and which is one of the prettiest and most beautiful instruments devised by any scientific man for some time. Here is also a candle, the rays from which are falling upon the radiometer. The radiometer consists of an aluminium cross, upon which are fixed four vanes, the whole being enclosed in an exhausted glass bulb. The vanes are also of aluminium, one side of them being lamp-blacked by exposure to flame, and the other side bright and plain. The action of the radiometer is due to the motion of the molecules of the residual gas in the bulb. The molecules are highly elastic, and they obey the laws of the impact of solid and elastic bodies. They beat against the sides of containing vessels with a force which is called *pressure*, and they beat against the face of any surface exposed to them with a force which becomes mechanical and evident when this surface is so arranged as to move and be unequally acted upon by the molecules on different sides. Radiant energy falling on a white surface is reflected off again without any change, but falling on a black surface it is absorbed and converted into thermometric heat. Whenever the temperature of a body is raised, it warms the molecules striking it, which move away from it with increased velocity; and, as action and reaction are always alike, in moving away they give the body a "kick," molecular pressure is produced, which, in the case of the vanes of the radiometer, causes rotation. It is a somewhat analogous phenomenon to that which we have seen in the "Little Marvel" steam-engine, in an ordinary lawn sprinkler, where water is scattered over a grass plot, and in the well-known electric fly. But in the radiometer

the external glass case plays an important feature. If the vanes were fixed, the external case would rotate. There is, in fact, a molecular wind between the two surfaces, blowing from the warm surface to the cold one. The existence of this bombardment of molecules has been incontestably proved by Mr. Crookes in that beautiful modification of his radiometer, the otheoscope, where the mind's eye can see clearly and unmistakably the little bullets striking the disc in an incessant shower from the lampblacked vanes. Here we have the vanes rotating in one direction, and the disc moving in the reverse direction. Two surfaces are always necessary to produce this radiometric effect. In the radiometer we have the vanes and containing glass, but in the air, at ordinary pressures, the opposing surface would be a cushion formed by the air itself.

If the vanes of the radiometer were fixed, and the rays from the candle were caused to fall intermittently on the radiometer, you would have all the conditions required to produce a motion that would result in sonorous vibration. If an ebonite disc were fixed inside the case, and intermittent light allowed to fall upon it, then, according to the theory of the radiometer, vibrations would be produced by molecular pressure, which would result in sound. That was the view taken by M. Mercadier when offering an explanation of the phenomena; but all the experiments made by Mr. Stroh and myself were purely negative, and showed that there may be an effect of molecular pressure, but, if there be, it is so small that it cannot account for these sonorous vibrations.

The next question is, Is it a function or case of vibration of the disc at all? Professor Hughes, who was present at the experiments, suggested that it might be the vibration of the air itself. To answer this question we immediately set to work. Mr. Graham Bell still maintains that the disc does vibrate, and will not admit that the experiments given in my paper before the Royal Society are conclusive, but adheres to his own original idea (for the present, at any rate), that it is the action of light upon the discs themselves, and will not admit that it is due to the expansion of air. But M. Mercadier, curiously enough, has shown that he got

the sounds just the same though the disc were cracked; and on my recent visit to Paris he was kind enough to give me a disc which is cracked in all directions, but nevertheless emits sounds as if it were intact. That must be sufficient to show that the sounds are not produced by the vibration of the disc, for we all know that cracked bells do not give the same sounds as when uncracked. Again, experiments show that, whether the disc be of ebonite, mica, zinc, paper, copper, or various other things, the sound given is always of the same *timbre*—of the same character of note. If the sounds were due to the vibration of the disc, different substances would emit different sounds.

We will now remove the disc from the case, and see if any difference is apparent.

Professor HUGHES: The sound seems louder.

Mr. PREECE: That is my experience—that the removal of the disc increases the loudness of the sound.

Professor HUGHES: If I squeeze the flexible tube I get no sound, which proves that if the air is cut off no sound passes.

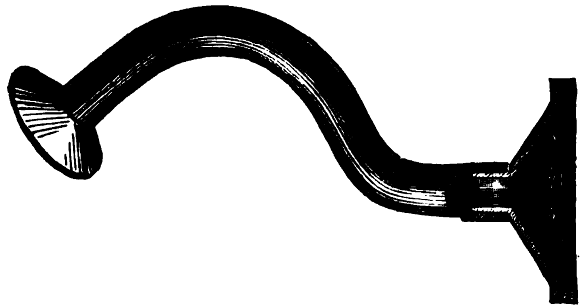
Mr. PREECE: That is a new experiment. Now, having shown that it is not the vibration of the disc,—that the sound is absolutely improved by the removal of the disc,—we have to ask ourselves whether it is due to the expansion of the air. In this particular case the air has been confined by a disc of glass; but this disc is a thick lens, so that it is scarcely possible to believe that the vibrations could have been caused by it. It is replaced by a thin glass, without any change in the sound.

We will now try the effect of gently removing the glass while the intermittent light is falling on the case.

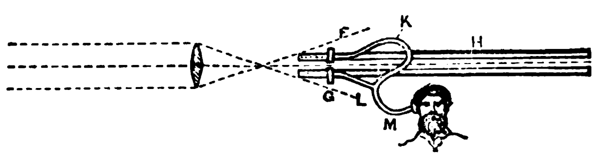
Professor HUGHES: The sound has ceased.

Mr. PREECE: Yes; the moment the confined air is allowed to expand in another direction by the removal of the glass, the sound ceases, so that really the sound can only be made evident as long as the air is confined. This experiment has been confirmed by M. Mercadier, and by Messrs. Bell and Tainter. Many kinds of cases were tried, conical, spherical, and rectangular; but the one best suited proved to be the form you now see.

The arrangement adopted in America by Messrs. Bell and Tainter is very similar to that now shown you, it being as follows :—



The mode of measuring the relative intensities of sounds emitted by different materials in different arrangements is very interesting. The instrument best adapted for the purpose is Professor Hughes' sonometer (such as the one before you), but, as the actual measurement by this instrument takes time, I will not delay you by using it: we will rely only upon Professor Hughes giving us, by ear, the approximate values. Professor Hughes' sonometer indicates, by a graduated scale extending from zero to 100, the different intensities of sound. Mr. Tainter has adopted a different arrangement to give the same comparisons. The following diagram will explain Mr. Tainter's plan :—



Two tubes (K) are connected to an india-rubber tube (M), at the end of which is an ear-piece. When the sounds emitted in two cases (one at the end of each tube) are of equal intensity, the comparison is simple; but if one sound is louder than the other, the case emitting it has to be moved further away, until a point of equal intensity is reached. Direct measurement is obtained by the ear, and the relative intensities are compared by the positions of the cases on a graduated scale.

Having shown that the sounds are due to the impact of waves of radiant energy, let us see if they cannot be improved. It is

quite evident that if the sounds be due to the expansion and contraction of the air contained in the case, then, if by any means we can in the same periods increase the temperature within that case, we shall increase the extent of those contractions and expansions. I have already explained that, when rays of radiant energy fall on lampblack, they are converted from radiant heat into thermometric heat. Now if we take a clean case (here is one) and attach it to the tube, we may assume that no sound is given; but Professor Hughes will perhaps kindly say.

Professor HUGHES: There is a very weak sound, of an intensity of about 5.

Mr. PREECE: That is owing to the case not being perfectly clean. If we now black the same case with fumes of burning camphor—[Mr. Stroh does so]—we will note the difference.

Professor HUGHES: I estimate the intensity of the sound now emitted at 50 or 45 increase above the last experiment.

Mr. PREECE: M. Mercadier and Mr. Sumner Tainter have, by totally different experiments, confirmed this effect of coating the case with lampblack. M. Mercadier employed a test tube, such as the one I now hold, which contains a piece of mica coated with lampblack. If the test tube be held in the path of the intermittent oxyhydrogen light, and the hearing tube attached to it, we shall get sounds.

Professor HUGHES: Yes; the sound is very intense, but is immediately cut off by squeezing the hearing tube.

Mr. PREECE: If we substitute a larger test tube containing a piece of wire gauze (for that containing the mica), covered with lampblack, as employed by Messrs. Bell and Tainter, I am in hopes that the sound will be sufficiently loud to be heard by all the members present. [This was so.]

Another form of the same kind of experiment is that of using a glass flask coated inside with lampblack. But Messrs. Bell and Tainter have gone still further, and, instead of using lampblack, they have placed worsted in the test tubes. I have here tubes containing worsted of different colours, and, as Bell and Tainter have shown, according to the quantity of heat which each of the colours ought to radiate from this degradation of radiant energy into

thermometric heat, so is the effect produced. We will try one or two of them.

The tube not containing any worsted, but lampblack, is now under experiment.

Professor HUGHES: I should think the sound is about 75.

Mr. PREECE: Blue worsted.

Professor HUGHES: 10.

Mr. PREECE: Green worsted.

Professor HUGHES: Say 13. The sounds are very light.

Mr. PREECE: Yellow worsted.

Professor HUGHES: 20.

Mr. PREECE: Red worsted.

Professor HUGHES: Not so loud as I should have expected: about 20.

Mr. PREECE: Black worsted.

Professor HUGHES: Very loud: 40 or 50.

Mr. PREECE: That series of experiments affords direct proof that the intensity of the sounds varies with the power which the substance has to convert the rays of radiant energy into thermometric heat. Mr. Graham Bell also proved that the loudest sounds were produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colours. He has experimented with sponge, spongy platinum, cotton, cork, and a host of other materials, and says that in some cases "the sound was so loud as to be actually painful to the ear."

Cannot these effects be produced by other means? Instead of using tubes, and flasks, and cases, let us take a little case which has inside it a spiral of thin platinum wire. If currents of electricity pass through that wire, for every current sent the wire becomes heated in direct proportion to the strength of the current passing, and if that be so, we ought to get the same effect as has just been obtained in the tubes. I have a glass tube here containing two wires, at the top of one of which there is a small coil of platinum wire. That coil is in an electric circuit, a wheel-brake being used for sending intermittent currents. The wheel-brake, or transmitter, is downstairs, and Mr. Kempe has now gone to turn it.

Professor ADAMS (listening): I can hear sounds, which go

higher as the wheel is more rapidly rotated; I can also feel that the tube is becoming hot.

Mr. PREECE: If we substitute an ordinary microphone transmitter, instead of the wheel-brake, we shall have a more delicate arrangement; and, if Mr. Kempe speaks to the transmitter, similar sounds will be heard from the tube, which is in reality a new kind of telephone.

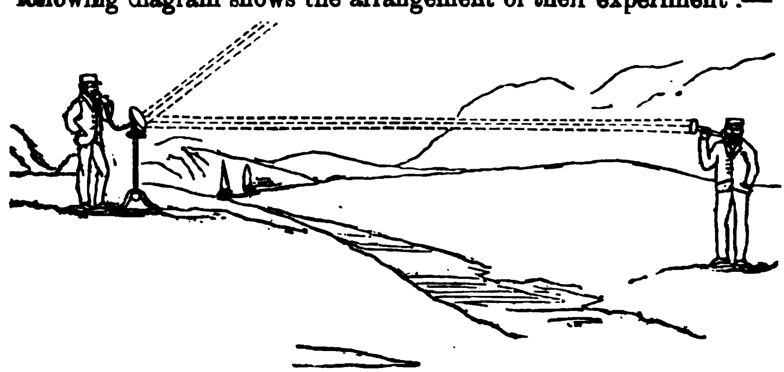
Professor ADAMS: Yes; I heard the names of the months, and numbers 1 to 20.

Mr. PREECE: That is a telephone based entirely upon the production of heat by currents of electricity and the sonorous vibrations induced by that heat.

To revert to the test tube experiments, I forgot to show you a very interesting experiment of M. Mercadier's. Instead of taking a disc having one series of holes, we use one having four series of holes in proper progression, and, when the rays of light fall intermittently from these holes on the test tube, instead of a note a chord is produced.

Professor HUGHES: I can hear sounds, but they only form an imperfect minor chord.

Mr. PREECE (continuing): Messrs. Bell and Mercadier have made one step in advance, and, with that curious simultaneity that often follows scientific men who are pursuing the same path, their results were published in Paris on Monday night last at the same time when they both announced that they had succeeded, independently of each other, in transmitting articulate speech by the aid of a lampblacked surface and the rays of the sun. The following diagram shows the arrangement of their experiment:—



It is a somewhat similar arrangement to that of the photophone. The rays of the sun fall upon a silver disc which, when vibrated by the voice, varies the form and character of the rays which fall upon a lampblacked surface at a distance. The distance is not given, but it must have been something beyond the reach of the ordinary voice. M. Mercadier, in a letter I have just received from him, says that "he presented to the Academy of Science a note on the reproduction of the voice by the aid of the themophone. At the same time there arrived a memoir from Graham Bell indicating that he had equally succeeded on his side in reproducing the voice without selenium and without electricity. We have therefore, both of us, without knowing it, at the same time arrived at the same result. It was the day after your visit that I made the experiment."

Mr. Graham Bell has also succeeded in replacing the selenium in his photophone by means of a lampblacked cell. He has coated glass with a thin coating of silver, and has scratched on the glass a zigzag curve, separating the glass into two parts, which are connected by means of a thin layer of lampblack. This arrangement he puts into the photophone in the same manner that he inserted the selenium cell, and by its aid has succeeded in hearing sounds and words. Mr. Shelford Bidwell (another earnest worker in this field) has also tried this experiment, but has not succeeded; Mr. Stroh has been equally unsuccessful; and the only way in which I can account for Graham Bell's success is, that he has been fortunate in making his experiments at Washington where the rays of the sun are more powerfully charged with radiant energy than those with which we have been able to experiment with.

I will only delay you a little longer, to explain to you the theory which I offer as accounting for these phenomena. I conceive that the cause of these phenomena must be common to them all, and a theory can only be satisfactory which will account for all of them without exception. Mr. Graham Bell in his last paper has attempted a theory which I am bound to confess I cannot accept, and I question very much whether it will be generally accepted. He looks upon lampblack as a sponge, which becomes enlarged when warm and contracted when cold; that as this sponge expands

and contracts it squeezes out air or allows air to come into it; and that it is the expansion and contraction of this spongy mass which throws the air into motion and produces sonorous vibrations. That theory Mr. Graham Bell has adopted and enlarged upon; but I must confess that I do not agree with it.

Starting from the radiometer, and from the theory that I gave you of that instrument, I want you to fix your mind's eye on the motion of the molecules.

In the radiometer we have an experiment that gives us ocular demonstration of the existence of the motion of molecules. The contact of the air on the lampblacked surface produces motion of that air. Here is a radiometer, the vanes of which are inclined at an angle, and above these vanes is affixed a flat disc of mica. The disc has no connection whatever with the vanes of the radiometer; but if there be a shower of molecules from the surface of those vanes, that shower of molecules must strike the flat disc, and cause it to rotate in an opposite direction to the motion of the vanes. [A candle was then brought near to the radiometer, containing the inclined vanes and disc of mica, and the result was as described—a rotation of the disc in a direction contrary to that of the vanes.] If any experiment could possibly carry conviction into men's minds, it is this beautiful one of Mr. Crookes'.

I say that when the rays of radiant energy fall on the black mass they convert it into a warmer substance, and in that condition the molecules of air strike it and rebound with increased velocity, thus producing greater pressure, which causes the air to expand. The air in expanding strikes the tympanum of the ear, which responds to these expansions, which are thus nothing more nor less than sonorous vibrations. If we fill (as indeed I ought to have done) one of the test tubes with the dark heavy smoke of camphor, precisely the same effect would have been noticed. The sounds would have been produced by the molecules of air striking the surface of the smoke and flying away with increased velocity.

Tobacco smoke under similar conditions gives similar results. If, instead of smoke, absorbent gases were used, the result would still be the same. Here is a clean flask out of which we will try to get sounds. If it be perfectly clean, and the air perfectly dry

we ought to get no sounds, because there is no surface upon which the rays of radiant energy can become converted into thermometric heat. [There were no sounds apparent.] I will now put some ammonia in the flask, and when the ammonia has evaporated the flask will contain vapour of ammonia, which will provide a substance for the rays of radiant energy to fall upon and be converted into thermometric heat, and sounds should be emitted.

Professor HUGHES: Yes; the sound is quite distinct now.

Mr. PREECE: By this theory we account in a very simple way for the remarkable property that vapour of water, of sulphuric acid, of ether, ammonia, and many others, as Dr. Tyndall has shown, are absorbents of heat rays.

The last instrument and fact I have to bring before you which Professor Bell has given to science is the *spectrophone*. He has succeeded, by the aid of this instrument, in exploring the ultra-red portion of the spectrum which has hitherto been beyond the reach of our senses. Intermittent rays of light pass through the collimator, and prism of a spectroscope, to a tube containing wire netting, lampblacked,—this being substituted for the eye-piece,—and by this means Mr. Graham Bell has been able to explore by the ear a portion of the spectrum that has hitherto been a *terra incog*. As far back as 1842, Dr. J. W. Draper, of New York, discovered that there was a dark region of the spectrum to be explored. He discovered three dark lines, which he called α, β, and γ. Last year Capt. Abney succeeded in photographing this region of the spectrum, and now Mr. Graham Bell has explored it with his spectrophone. His results show that the maximum sound is given in different parts of the spectrum according to the quality and character of the material used. For instance, he finds with lampblack (as we have found in England) that the maximum sound is given amongst the ultra-red rays low down in the spectrum. With red worsted he finds that the maximum effect is given in the green, and it is just in that part of the green spectrum where the red becomes black. If any of you have ever moved a red subject in the field of a spectrum, you will have found that when you got into the green the red became black, and there it gives the maximum sound. Just in the same way, where you take a green substance

and move it in the spectrum, you will find that in the red portion it becomes black, and there it gives the maximum sound. The following is the table of the distribution of heat in the spectrum arrived at by Bell's exploration:—

Lamp Black	maximum	Ultra-red.
Red Worsted	„	Green.
Sulphuric Ether	„	Red.
Iodine	„	Green.
Peroxide of N.	„	Blue.
Selenium	„	Red.

With regard to these experiments, Mr. Graham Bell says: "Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that the nature of the rays that produce sonorous effects in different substances depends upon the nature of the substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body."

Professor W. G. ADAMS: I need scarcely ask you to pass a vote of thanks to Mr. Preece for his communication, seeing that you have already signified your approval by the way you have received it. Mr. Preece has given us an account of the principal results which have been arrived at by various investigators, and in his last few remarks has given us some very interesting facts which Profestor Bell has discovered with regard to the spectrum. In the copy of *Nature* which is published to-day will be found an account of Professor Bell's latest investigations on this subject. I will not further occupy your time with any remarks of my own, but invite discussion on this interesting subject.

Dr. MOSER: As time has very much advanced, I shall mention only the result of investigations I have made with the selenium photophone. I melted selenium on copper plates, and annealed it; in short, I treated it in the same manner in which I had obtained photophonic cells yielding a good sound. Then I observed that the selenium, or rather the Cu_2 Se, split off from the copper plate, so that there remained between the copper and the selenium only a

very imperfect and slight contact. In the selenium cell this contact is influenced by radiation, and thus the selenium photophone is nothing else than a microphone. It is most similar to that form of the microphone which has been described as the thermoscope. I do not deny that radiation will influence the conductivity and the energy of the selenium in its four allotropic modifications. But in the essential part the efficiency of the photophonic selenium receiver is the same as that of any other microphonic (thermoscopic) receiver. As to the radiophonic phenomena, I see no reason to separate the selenium from all other bodies. I think we have to join it rather to all these bodies, the effect of radiation on which Mr. Preece has just now so clearly demonstrated, and thus, I believe, there is no hope of discovering any unknown power or new relation of forces in the selenium.

Mr. R. E. CROMPTON: I should like to ask Mr. Preece if he really means that the intermittent radiance is communicated directly to the air, and not indirectly by means of the walls of the confined chamber, one side of which is a piece of glass? Does he mean that the molecular disturbance which takes place in the walls of the chamber does not play the leading part in producing the sound that we hear in the ear-piece?

Again, Mr. Preece says that no measurable expansion of the substance receiving the intermittent radiance has yet been observed. No doubt the molecular movements giving the sounds can hardly be dignified by the terms of expansions and contractions, which latter movements are the sum of the increased or diminished amplitude of swing of the whole of the molecules forming the substance. It is noticeable that the substances which appear to give the loudest sounds are those which appear to conduct heat—in other words, communicate the heat vibrations the slowest. The molecular movements we are now discussing may be movements simply of the surface molecules, may be very considerable, and yet be utterly unobservable by any instruments we have at present. One would imagine that the first effect of intermittent radiance striking on the surface of the diaphragm would be that the molecules on its surface would have the swing of their motion amplified, and

that this alone would be quite sufficient to communicate vibratory movement to the air, without the molecules throughout the body of the substance having to be set in motion; and it is not until the amplitude of the whole of the molecules is increased that measurable expansion can at all be obtained.

Mr. W. H. PREECE: With regard to the question asked by Mr. Crompton, I had hoped that I had made it perfectly clear that my theory was that the walls of the air space played the fundamental feature in the phenomena, and that the rays of radiant energy which passed through, without having the least effect or influence directly on the air molecules, converted the walls of the containing space into a warm surface, and that, owing to the warming of that surface, greater motion was imparted to the molecules of the air in contact, which caused variations of pressure, and so produced sonorous vibrations. Seeing that the first essential feature of this production of sonorous vibrations is that there shall be a surface, whether it be the lampblacked interior surface of a wooden case, whether it be the worsted placed inside the tube, whether it be the lampblack deposited upon the side of a flask, or whether it be a wire gauze coated with lampblack, the existence of lampblack, or some such absorbent matter, is the fundamental feature of the phenomena—it is the spot where the radiant heat is converted into thermometric heat, and where the play of the molecules is produced that results in sonorous vibrations.

Next, as regards the measurement of molecular motions. The most delicate, the most sensitive instrument that we possess for the detection of minute motions is the microphone. The microphone, as you all have heard, has succeeded in making the motion of a fly appear like the tramp of a horse across a bridge. That may have been the mere exaggeration of a penny-a-liner, but the fact is that whatever motion exists to produce sonorous vibration is detectable by the microphone. We have not succeeded, by the most delicate tests, in proving the existence of any molecular motion in any hard substance we have experimented upon, but Professor Bell has. Professor Bell has succeeded with a microphone—a Blake transmitter—in obtaining sounds, but that was due no doubt, as I have

said, to the intense rays of the sun at the time of the experiment. I have not the slightest doubt whatever that there is vibration in these diaphragms (I have said so in the paper I read before the Royal Society), but what I do say is that the sounds which I have rendered evident to you to-night are not due to the vibration of diaphragms, but are simply due to the expansion and contraction of air, owing to the formation of heat on the wall space that contains that air.

A hearty vote of thanks was accorded to Mr. Preece for his communication.

A ballot then took place, at which the following gentlemen were elected

Members :

Professor James Dewar, F.R.S.
Richard Pentney Eidsforth.
James Reid.
W. H. Stone, M.D.
J. W. Swan.

Associates :

William Blanch Brain.
Arther Butler.
Medmer Goodwin.
J. Gladwyn Jebb.
Frederick C. Phillips.
Charles Heaton Sharples.
George G. Uren.

The Hundred and Second Ordinary General Meeting of the Society was held on Thursday evening, May 26th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. Foster, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of new candidates were read and suspended.

The President: Before calling upon Mr. Luke to read the paper of the evening, I will ask the Secretary to read a letter which has been received from General Sir Frederick Roberts, whom we hoped would have been with us to-night.

The Secretary then read the following letter:—

"United Service Club,
"Pall Mall, S.W.,
"19*th May*, 1881.

"Dear Sir,—Will you do me the favor to thank the President and Council of the Society of Telegraph Engineers and of Electricians, for inviting me to hear Mr. Luke read his paper on Thursday evening, the 26th inst.

"I much regret that a previous engagement prevents my being able to accept the invitation.

"I am the more sorry for this, as the subject is one in which I am deeply interested, and Mr. Luke is a gentleman I am much indebted to for the able assistance he afforded me as Head of the Telegraph Department in Afghanistan.—I remain, dear Sir, yours very truly, "Fred. Roberts."

ON THE CONSTRUCTION AND WORKING OF A MILITARY FIELD TELEGRAPH (BASED UPON EXPERIENCE GAINED DURING THE CAMPAIGN IN AFGHANISTAN IN 1878-79-80).

By S. P. V. Luke, C.I.E., Member.

Before entering upon the general question of a military field telegraph, a short account of the work done in connection with the electric telegraph in Afghanistan during the recent campaigns,

from which the experience embodied in this paper has been gained, may prove of interest.

It will be remembered that, on the declaration of war against the Amir of Kabul, our army invaded his country by three different routes, known as the Kandahar, Koorum Valley, and Khyber routes respectively. A field telegraph accompanied the column advancing along each of them, and the total length of telegraph line constructed beyond the frontiers, in connection with the military operations, by the Government Telegraph Department, amounted to very nearly 500 miles.

As, however, my charge was limited to the Khyber line, my remarks are confined to the telegraph constructed from Peshawur to Kabul through the Khyber Pass, a distance of 180 miles.

The war was commenced by the taking of Ali Musjid on the 21st November, 1878; but, owing to the unsettled state of the country and the hostility of the Afreedi tribes, it was not until the 26th December that permission was given to commence to lay the telegraph. Three days after, an office was opened at Ali Musjid, but it was an unfortunate start, for the very next night the line was totally destroyed by the Afreedis for 5 miles, and the wire carried off. This caused further delay until better arrangements for the protection of the line could be made; but on the 8th January, 1879, a more successful attempt was made, and by the 29th the telegraph was taken, *via* Lundi Kotal and Dakka, to Bosāwul, 53 miles from Peshawur.

Meanwhile the Bengal Sappers and Miners had taken all their telegraph train right through to Jelalabad, and, working back, met my line at Bosāwul. The construction of the semi-permanent line was, however, continued on to Jelalabad, in order that the Sappers' lighter material might be set free for use further on in case of an advance.

Jelalabad was the terminus of the telegraph until April, when it was continued on to Gundamak, 117 miles from Peshawur. On the signature of the treaty of Gundamak, in June, 1879, and the withdrawal of the troops, the wire was rolled up as far back as Lundi Kotal, and all the telegraph material brought back to Peshawur, except the supports, which were abandoned for want of carriage.

During this first campaign the telegraph was working for about six months; and some idea of the annoyance caused by tribes, and of the difficulty of keeping up communication, may be formed when it is stated that the wire was cut 98 times, and about 60 miles, on a total length of 117 miles, were stolen and never recovered. What they did with all this wire it is hard to say, except that they carried it away to distant villages. Occasionally it was cut up into slugs and fired into our camps.

War broke out again on the murder of Sir Louis Cavagnari and Embassy in September, 1879; and on the 3rd October the construction of a telegraph by the Khyber route was again taken in hand, beginning this time from Lundi Kotal, up to which point the line constructed during the first campaign had remained in operation.

On the opening of the second campaign, there was nothing to delay the start: the material brought back from the first campaign was available, and the country, as far as Gundamak, was well known and tolerably safe.

The construction was pushed on with all possible speed, and on the evening of the 21st October I met the Sapper line, the material for which had been carried forward as before, nine miles out of Jelalabad. At the end of a month from starting, the line was finished to Gundamak, a distance of 80 miles from Lundi Kotal, notwithstanding considerable delay being caused by difficulty in procuring poles.

Meanwhile an officer of the Telegraph Department, Mr. Josephs, had accompanied General Roberts in his advance on Kabul, *via* the Koorum Valley, and commenced laying the line from the Kabul end.

On the 19th November, a junction between the two lines was effected at a place called Jagdalak, and through communication between India and Kabul established.

From this time the line worked almost uninterruptedly until the commencement of the siege of Sherpur. On the 11th, 12th, and 13th December, there was severe fighting round Kabul, and on the 14th all our troops retired into the Sherpur cantonment, and the siege began; but, curiously enough, it was not until the

morning of the 15th that the wire was cut. Notwithstanding that fighting was going on all round, and that thousands of the enemy were passing under the line, communication with India was carried on successfully. All the urgent telegrams were sent off, and the Kabul office was quite clear of work on the night of the 14th. This was probably because the enemy did not understand or know the value of the telegraph.

Early on the 15th the wire was cut, and it was afterwards ascertained that the line was entirely wrecked as far back as Pezwan, a distance of nearly 50 miles.

During the siege a wire was erected all round the Sherpur cantonment, and six telegraph offices were kept at work, affording a means of rapid communication between headquarters and divisional and brigade officers.

Directly the siege was over and the enemy dispersed, the reconstruction of the line destroyed was commenced. At this time the snow lay thick, and the ground was hard as iron from the frost, rendering the putting up of the line no easy matter, especially over the Latabund Kotal, a pass 8,000 feet high, but, notwithstanding all difficulties, through communication with India was re-established on the 8th January.

There was not so much annoyance from wire-cutting as during the first campaign, but, nevertheless, the wire was cut 50 times, and about 57 miles stolen.

On the retirement from Kabul, the wire was rolled up, and all the material brought back to India, and there is now no telegraph office beyond Peshawur.

We now come to the general consideration of the construction and working of a field telegraph, based upon the experience gained in the operations just briefly described.

Mr. Preece, in a lecture delivered at Chatham some years ago, on military telegraphs, divided them into four classes, viz. :—

(1.) The Permanent Line.
(2.) „ Semi-permanent Line.
(3.) „ Flying Line.
(4.) Visual Signalling.

(1.) The Permanent Line he defined to be a continuation of the commercial system of the country in which the operations are taking place; or of the establishment of a similar system in the event of there being none, or of the original one being totally destroyed.

(2.) The Semi-permanent line he defined to be that line which maintains headquarters in its advance in communication with the permanent line; and

(3.) The Flying Line to be the telegraph connecting headquarters with its divisions.

Owing to the great perfection to which visual signalling has been brought of late, the necessity for the Flying Line in a hilly country like Afghanistan, where the sun is rarely obscured for many days together, no longer exists. By means of the heliograph, direct communication can be kept up between points 40 or more miles apart, and messages sent at the rate of from six to eight words per minute. In an enemy's country the heliograph has enormous advantages over the electric telegraph. It cannot be cut and destroyed like the wire; it can accompany the army, no matter how rapidly it advances; it is easily carried, and can be set working in five minutes; it can be protected and worked even whilst a battle is going on all round.

Its great disadvantage, viz., that it can be read by enemies as well as friends, made no difference in Afghanistan, as its flashes conveyed nothing to the minds of the Afghans, and only excited their wonder and astonishment.

Visual signalling being so perfect, there is not the necessity for the hurried construction of the field telegraph which there would be if no other means of communication were possible between the army as it advanced and its base of operations. The field telegraph may now, perhaps, be defined as the line that, starting from the base of operations, where it is in connection with the regular telegraphic system, is taken into the enemy's country as soon after the advance of the army as the state of the country admits. It is little or no use constructing a line till arrangements can be made for its protection.

Since the hurried and rapid construction of the telegraph is

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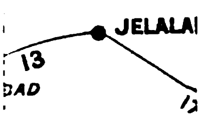

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not essential, there is no necessity for the use of very light material which would afterwards require to be replaced, but a strong semi-permanent line can at once be put up, calculated to last at any rate as long as the campaign is likely to do, and which can be made permanent by addition or alteration without being entirely reconstructed.

There are two kinds of field line to choose from—

The Ground Line.

The Overhead Line.

A ground line was tried by the Bengal Sappers and Miners during the first campaign. An insulated wire, protected with plaited hemp, was laid on the ground alongside the road for the greater part of the way between Jelalabad and Dakka, a distance of 30 miles. It was not buried in any way, except where it crossed a road. This line never lived for an hour. It was cut in 20 or more places every day. The temptation was too great. Passers-by, seeing a black cord running by the roadside, had their curiosity excited to know what it was, and nothing was easier than to hammer the cable between two stones till it was severed. Whether this line would have answered better had it been buried is questionable. Unless the burying had been done secretly at night, some people must have seen what was going on, and would have dug it up out of mischief or malice. Then, again, if a fault occurred in the wire itself, a thing always probable, the difficulty of localising it in a buried wire is very great, and the trouble to repair it immense.

All things considered, it may, I think, be admitted that a ground line is inferior in every way to an overhead one for a field telegraph such as defined above, and I will now proceed to the description of an overhead line, embodying the results obtained by experience in Afghanistan.

First, as regards line material:

For posts it is undoubtedly best to rely as much as possible upon the resources of the country through which the line has to be made. If it be known beforehand that no timber of any kind can be procured locally, then it becomes necessary to collect poles of some sort at the base of operations. But it is extremely improb-

able that an invading army would traverse a country utterly devoid of timber of some kind. Take the Peshawur to Kabul line as an example. From Peshawur, through the Khyber Pass to Dakka, where the Kabul river is struck, a distance of 40 miles, not a stick of any kind that could be used for a pole can be got. From Dakka onwards no great difficulty was experienced in getting poles. Good deodar and pine saplings were either procurable from villages under the Safed Koh, or, coming down the streams in rafts, could be purchased at places along the banks of the Kabul river. At Kabul itself, any quantity of excellent poplar poles, admirably adapted for a telegraph, were procurable.

But whatever the resources of the country through which the line will pass, it is advisable, in order to avoid any delay at starting, to have at least 10 or 12 miles of poles ready at the base of operations, so that the line can be commenced at a moment's notice, and, whilst the first 10 miles are being put up, arrangements can be made for a further supply.

The question is what is the best kind of pole to have. For the line from Peshawur towards Kabul a quantity of bamboos were collected at Peshawur, averaging about 20 feet long and about 3 inches diameter at the butt, rough and untrimmed, and were found to answer admirably.

In a country where there are no roads and no wheel carriage, the question of transport is the first one to be considered, and it may be taken for granted that men, camels, ponies, or mules are the only means available. A camel will carry poles in 10 feet lengths and a mule in 7 feet, but the shorter the better. Then, again, all animals will *drag* poles, which is perhaps a better way than carrying them, and then the length up to 20 feet is immaterial.

A *jointed* pole of any kind does not seem to answer, except for a very light wire, say, 150 lbs. to the mile. The joint is always a weak point.

If bamboos are used at all, they should be not less than 18 or 20 feet long, in one piece. There is no great object in their being perfectly straight, and they are best only roughly trimmed. Bamboos of this description, planted 20 to the mile, with every fourth support formed of two bamboos erected like shears, make a

very strong and durable line, capable of carrying a 300-lb. wire with ease. The objection to them is difficulty of transport, owing to their length, but they can be dragged by camels or mules or donkeys, or carried by porters. They have the advantage of being cheap, and in a dry country like Afghanistan, where there are no white ants, will last six months at the least. Some bamboos used in the Khyber Pass were found quite good after being up for a year.

Besides bamboos, there are what are known in India as "bullies"—that is, young trees or saplings of teak, pine, deodar, poplar, and other wood—to be considered. In selecting, the point is to combine strength and durability as much as possible with lightness. For a military field line big heavy poles are out of the question: they cannot be carried, and it is better to have a larger number of smaller poles. Where height is required, two small poles can always be lashed together with wire, such a joint being perfectly reliable. If the poles are too weak for use singly, they can be used two together like shears, which gives great strength.

It may happen that the telegraph line goes through a wooded country, in which case the wire should be supported as much as possible on the trees. The best way to do this is to nail a bracket of dry wood to the tree, and attach the wire to it by screwing another piece of dry wood over it, taking care to have the tree inside the wire at angles. Another way is to fasten a stirrup of dry wood with wire to a branch, and bind the line wire to the centre of the stirrup, it being always understood that insulators of any kind are out of place in a dry country like Afghanistan, as being unnecessary, and making extra weight to carry.

The only case where insulators seem desirable is where rocks are met with so precipitous that spikes can be driven into them, and the wire supported on insulators attached to the spikes. Such rocks, however, will rarely occur. About a dozen rock insulators were utilised in the Khyber Pass.

The wire used for the field lines in Afghanistan was of galvanised iron, weighing 300 lbs. per mile, or No. 12 Indian gauge, equal to No. 9 (about) B.W.G. This answered admirably, but is perhaps unnecessarily heavy. For the distance that a field line is ever likely to extend to, or (say) up to 300 miles from base

of operations, a wire weighing 200 lbs. per mile would answer every purpose. A wire of this size has great advantages. It should be in coils weighing 100 lbs. each, so that a good mule or pony will carry 2 coils, or 1 mile, and a camel 4 coils, or 2 miles. It is easy to manipulate and joint, and a block and tackle, or other appliances for straining, are not required. The diameter of all coils should be exactly the same, to avoid delay in placing them on the wire reel. It is a good plan to have each coil wrapped round with a band of tarred canvas, to preserve the wire from injury during transport. An ordinary galvanised iron wire, weighing 150 lbs. to the mile, answers very well for short distances; but it does not give a sufficient margin of strength, to stand accidental blows, for a semi-permanent line. If a camel breaks down a post, the wire will most likely break too.

In the matter of *Tools*, the digging tools should be adapted to the habits of the people who have to use them. A good wire reel for paying out wire, strong and portable, is essential. It should be so constructed that it can either remain stationary and the wire be dragged out from it, or be carried on men's shoulders and the wire *paid out*, according to the nature of the ground. It should also be fitted with a crank handle, and be adapted for quickly coiling up the wire when taking down a line.

Paying out wire from the backs of animals is not to be relied on in a difficult country, and it is improbable that the roads will admit of carriages of any kind for the purpose.

For linemen engaged in the construction of the line, a tent of the pattern known in the Indian Ordnance Department as the "Followers' Pâl" was found to be the best for wet, heat, and cold. It should be made of three cotton cloths, the outside one white and inside one blue. It should open at one end only, the door to lace-up with brass eyelet holes; poles of male bamboo. The dimensions are as follows: 12′ long, 10′ wide, 6′ 6″ high, 1′ 6″ walls, three upright poles each 6′ 6″, two ridge poles each 6′, twenty-six wooden or iron pegs, and one mallet. This tent will hold 12 natives, and weighs 120 lbs. A good mule or pony will carry two of these tents complete, or shelter for 24 men.

The "Followers' Pâl" also answers very well for a field telegraph office. In the inner half of the tent is room for two signallers' cots, so that two soldiers can sleep there, while in the outer half there is room for the office table, batteries, boxes, etc. It is too hot for Europeans in the hot weather, but, if the office is to remain stationary for any time, a thatched roof can be built over it, which makes it cooler than a double fly tent. Of course, if an office is an important one, and required for a length of time, a much larger tent, or a building of some kind, will be necessary, but for a field telegraph office proper, the tent described will answer perfectly.

An admirable little tent, known as the "Officers' Servants' Pâl," was made by the Elgin Mills Company, Cawnpore. It will hold two natives comfortably, and was found well adapted for the linemen attached to a telegraph office for interruption and repair duty. It only weighs 32 lbs. and is very strong and serviceable.*

Next, as regards "office equipment:"

In India the "sounder" is the only instrument used, and for a field telegraph it leaves nothing to be desired. Three kinds of sounder were sent up to Afghanistan: an ordinary Siemens' sounder with relay, for use at the terminal and important intermediate offices; a direct working sounder, for small offices; and a portable sounder, for interruption duty and opening out anywhere on the line.

The "lightning dischargers" used were Siemens' brass-plate pattern for two lines, fitted into a mahogany case to keep out the dust.

The alarm bell was of the ordinary trembling pattern. This, with a vertical galvanoscope and a portable American clock, completed the necessary instruments.

The batteries sent for use in Afghanistan were—

The ordinary Minotto form of Daniell's cell as used throughout India.

A box divided into twelve compartments, each fitted up as a Minotto cell.

* A photograph was shown of these tents, and also of the tools used in Afghanistan.

The patent Leclanché portable battery, made by the India Rubber, Gutta Percha, and Telegraph Works Company.

A small portable battery, for use with the portable sounder.

All these, except the last, had their disadvantages. The first was too heavy and cumbersome; the second almost invariably leaked; the third was found to get broken and injured in transit. The solution of the question of the best battery for a field telegraph was certainly not arrived at. The special points to be considered are lightness and portability, strength, and consequent immunity from injury in transport: the battery should come quickly into action, and, above all, be easily cleaned and renewed.

The plan adopted for supplying offices during the Afghan campaign with *petty stores and stationery* was to have two boxes, each measuring 2′ 3″ × 1′ 2″ × 1′ 4″, for each office: these two boxes form one mule-load, and contain, the one a supply of stationery and printed forms, and the other petty stores, such as *office tools, message draft box, eyelet press, small copper earth plate*, etc. The stationery and forms required depend upon the system of working in vogue in the country of which the field telegraph is an extension. The point to be borne in mind is to keep separate the things that are not quickly expended and those that are. For instance, a set of office tools—consisting of *cutting pliers, screw-drivers (large and small), wedge for opening packing cases, knife, sand-paper, round-nose pliers, gimlet to bore holes for connecting wire*—will last the whole campaign, as will the *message box, eyelet press, letter clip*, etc. The *first* equipment supplied to each office should contain all these; and at the base of operations supplementary equipments, containing only *message forms, envelopes*, and other articles quickly expended, should be kept in readiness to issue to each office as its stock becomes exhausted.

A folding *camp table*, 3′ 6″ × 2′ 6″, with teak-wood frame and deal top, answers as well as anything. Small offices require two, and larger offices according to the number of instruments in use. Generally speaking, one table is required for each instrument, *plus* one for the signaller in charge to write at. It is better to have a separate table for each instrument than to have large tables.

For large offices, *stools* of some portable kind are required; at small ones, the stationery equipment boxes can be used to sit on.

The weight of one complete office equipment, including the office and linemen's tents, as described above, is 800 lbs. The regulation allowance is 160 lbs. per mule; hence it required five mules to carry the equipment for one office, but, as in the case of the wire, this weight could perhaps be reduced on a future occasion.

As soon as it has been decided to erect a field telegraph, the first thing to do is to establish an agency at the base of operations, for receiving and forwarding stores. A responsible officer should be placed in charge, and to him should be consigned all the telegraph material required for the campaign. On arrival of the stores at the base agency, they should be considered to have reached their destination, and receipts for them should be given by the forwarding agent.

The simplest, if not the only plan, to keep the accounts of the stores of a field telegraph, is to write off all stores against the work as soon as they reach the base of operations, and, on the conclusion of the campaign and dismantlement of the line, to credit the work with all stores brought back to or remaining at the base.

Having described the equipments of all kinds used, and supposing them to have reached the base of operations, the next point is the carriage beyond the base.

Starting, as it probably does, with an indefinite amount of work before it, it will be impossible for the field telegraph to provide *all* its own carriage. It must rely in great measure upon the transport department, and on whatever local means may be available. But it is very necessary that the field telegraph should have *some* carriage of its own, and the want of it was very much felt in the Afghan campaign.

It being impossible, as remarked above, to keep up an indefinite quantity of carriage, the best plan is to keep the smallest amount actually required, that is to say, sufficient for the carriage of the working party and their tools, and for one unit of material, the unit of material being taken as 10 miles of line and one office:

this is not to include the carriage of posts, which must be provided for specially.

Taking the strength of a working party at 1 Inspector and 50 men, the weight of their tents, tools, clothes, and one unit of material, with 3 days' rations, will amount to 7,520 pounds, and converting this into mule carriage at 2 maunds per mule, 47 mules would be required. With 47 mules, at the rate of 1 driver to 3 mules, 16 men will have to go, and will require rations for themselves, as well as for the animals, requiring 4 more mules, or (say) 5, allowing for the carriage of their own rations. This brings the total of mules required for a working party and one unit of material up to 52, allowing *none* spare: allowing 10 per cent. spare for sickness and sore backs, we arrive at a total of 57 mules. But, as before remarked, the weight of the equipment can easily be reduced, and wire used weighing 200 lbs. per mile instead of 300 lbs.; besides which, good mules will easily carry considerably more than 2 maunds, or 160 lbs. Taking these points into consideration, a properly equipped field telegraph train of 50 mules will suffice, always supposing that the heavy carriage and distribution at different points along the line will be done by the transport department or other local means. Fifty mules is, therefore, the amount of standing carriage which should be kept up for the field telegraph department.

The question of the equipment of these animals is of the first importance. Unless fitted with proper gear, they will get sore backs after the first day's work. The experience gained in the Afghan campaign has shown that the best pack-saddle is that known as the "Ordnance" pattern, manufactured at Cawnpore. This is better than the "Otago" pattern, which is perhaps the next best. But even the "Ordnance" saddle has disadvantages. Each animal requires to be fitted with its own particular saddle, while even if a mule gets out of condition the saddle is likely to give a sore back. A universal adjustable saddle which will fit any animal has yet to be designed.

The question of camels has not been entered into, because they would never do for standing carriage, mules or ponies are so far superior. If nothing but camels can be procured, one camel

may be calculated to carry as much as two mules. They are very slow compared to mules, more difficult to manage, and will not stand a cold climate, at least the Indian camel will not.

Loading the animals is a most important duty, and one likely to be carelessly performed. The men should be practised at it before starting, to accustom them to load properly and also quickly, for a great deal of time is generally lost in the operation. One point very difficult to attain with natives is to get them to make up the same loads daily. If left to themselves they will never load an animal twice alike.

Next, as regards the "working party:"

The great point is, taking into consideration the difficulty of commissariat and transport in an enemy's country, to have as small a party as is consistent with efficiency. A working party to erect a field line in India would be composed somewhat as under:—

1 officer in charge of the work.
1 Inspector in charge of the party.
6 trained linemen.
44 coolies or ordinary labourers.

Distribution of poles is not provided for in this estimate; it is supposed to be done by local labour, or animals of some sort.

Instead of the ordinary labourers, it would of course be better to have all trained workmen, but this is generally impracticable.

The Inspector should be mounted, and look after the party generally, especially the distribution of the material.

It may be possible to get a working party from a regiment on service. In Afghanistan the native soldiers, especially the Sikhs, did capital work; moreover, they liked the work, getting of course extra pay. Men of the "pioneer" regiments are specially well adapted for telegraph work, accustomed as they are to the use of tools. If 50 men of a pioneer regiment can be spared for the field telegraph, nothing could be better. It would be a good thing if erecting a field telegraph were made a special feature in the drill of a pioneer regiment.

If military labour cannot be obtained, men must be got from villages along the route, but this does not answer well, for the villagers will never go far from their homes, and new men have to

be put on the work daily. They, therefore, never learn their work properly, and the constant and close supervision thereby necessitated is a cause of delay.

The linemen and any labourers *permanently* engaged should be properly fitted out with clothing and shoes before they cross the frontier. The weight allowed each man for his cooking pots and clothes other than what he carries on his back should be limited to 30 lbs. Every man should be armed with a short sword, and Inspectors should be supplied with revolvers.

The party of 1 Inspector and 50 men, as described above, is calculated to put up from 3 to 4 miles of line a day. Taking into consideration that in an enemy's country the working party cannot camp out between the fixed military posts, which are from 10 to 12 miles apart, and therefore have a long distance to go and return from work; that the nature of the country is often difficult for a telegraph line; and that delays occur in the supply and distribution of poles, 90 miles per month for a semi-permanent line may be considered good work: 5, 6, or more miles a day may occasionally be done when the country is open and the digging easy, but that is exceptional, and an average of 3 miles a day is all that can be counted on, with the strength of party as given.

The first thing to be done is the marking out, which is generally the work of the officer in charge. For alignment, the great point is to keep close to the road or track, of course cutting off sharp angles, but on no account should the wire be out of sight from the road. The reason for this is obvious, patrolling and inspection being borne in mind. The posts should not be planted so close to the road as to endanger their being knocked down by camels or carts. They should be erected as much as possible in spots not easily reached by animals, and, in a hilly country, above rather than below the road. Every advantage should be taken of the nature of the ground to reduce the number of supports, by making long spans wherever practicable. For an uninsulated line, struts should always be used at angles, and never stays. Besides the fact that wire stays act as faults in wet weather, they are not easily seen, and camels go blundering up against them, and perhaps bring the post down. For joints in wire of the size used, the

"twisted" is the best and quickest made. There is no occasion to solder joints on a semi-permanent line. On the Peshawur to Kabul line no bad communication ever resulted from the joints not being soldered. For attaching the line wire to the posts, stranded binding wire is the best, being more pliable than ordinary wire. A good plan, if time will allow, is to cut a small nick in the side of the post, about 6 inches from the top, in which to rest the wire, and then bind it firmly there. If tightly bound, the nick may be dispensed with. If large rivers are likely to be met with, special arrangements for crossing them should be made: spans are preferable to insulated wire cables. In a hilly country, where the nature of the ground permits, very long spans can be made with safety. One span at Kabul with ¾ mile of wire in it never gave any trouble.

When the line is once up, the question in an enemy's country is how to protect it. That this is an important question will be gathered from what has already been said, viz., that the wire was cut 98 times in the first, and 50 times in the second campaign, and over 100 miles of wire in all carried off between Peshawur and Kabul alone. This question of protecting the line seems to divide itself under two heads, viz.:—

(1.) How to keep the people from cutting the wire.
(2.) How to catch the people who cut it.

Under the first head comes the plan of subsidising the tribes, through whose country the line passes, to protect the wire. This answered fairly well in certain districts, but was by no means generally successful. Then there is the method of making certain villages responsible for the safety of the wire in their particular district, and fining them whenever it is cut. But the villages are often few and far between, and the excuse of the head men is, "We have enemies, and they have come and cut the wire in our neighbourhood in order to get us into trouble." This plan would doubtless answer if each case of wire-cutting was followed by severe measures, such as hanging the head man or burning the village responsible, but such severity would be looked upon as an atrocity. In a country like Afghanistan, infested by robbers, in

which our influence did not extend half-a-mile on either side of the road, the best means of protecting the wire has yet to be devised.

Secondly, regarding catching the people who cut the wire. During the recent campaign the wire was never cut during the day, but always at night. One plan is to patrol the places where the line is frequently cut, but this duty is very harassing for the troops. On one occasion only was the patrol successful. One night during the second campaign, near the village of Batikot, a patrol party concealed themselves. The marauders came, and the soldiers killed two and wounded ten or twelve. This had a very good effect, but only a temporary one. The question of laying traps to catch the marauders has also been gone into. One arrangement was to put a permanent current on the line between two offices, in such a manner that the cutting of the wire caused a bell to ring in each office. Sowars, or native cavalry, were ready to go out from each end at a moment's notice to intercept the thieves, or at any rate prevent their carrying off much wire. The great objection to this plan was that it stopped the through communication on the line all night, and with a heavy message traffic it could only be carried out at the expense of great delay to messages. The idea of having some kind of torpedo connected with a post, which would explode when the post was interfered with, was mooted, but this threatened as much danger to friends as enemies, and was not tried. This point of how to catch the men who cut the wire is the one of all others that requires suggestions and plans, and it is to be hoped that before another campaign some means for doing so may be devised. Admitting that the line will be frequently cut, it becomes necessary to make arrangements for its speedy repair. In order to do this, it is necessary to have offices not more than 10 or 12 miles apart, that is, at every military post. This was done during the recent campaign, and at each office two trained linemen were stationed, with complete sets of line tools and a fixed stock of spare wire and poles. On the Peshawur to Kabul line the quantity of spare wire was five miles for each office.

When an interruption occurred, the officer commanding the station on each side of the break was applied to (notice being

given over night if possible) for a sufficient guard, and a certain number of porters to carry spare wire, poles, etc., to attend the telegraph office at daybreak. The linemen, with porters and guard, started, as soon as it was light, along the line from either end. The time taken in restoring communication of course depended on the damage done. If anything less than a mile of wire was removed, the line was probably repaired in a few hours: only very rarely did interruptions last over one day. Fortunately, as a rule the posts were not stolen, but merely pulled down. The robbers seemed to care only for the wire. The question of having mounted men, provided with light copper wire, to repair the line temporarily was considered, but involving as it did an additional and mounted escort, and an establishment of ponies, and specially trained men, the plan was not adopted, nor would it have been worth the extra trouble and expense. In addition to going out to restore the line when interrupted, it is the duty of the linemen to patrol the line half-way to the next office on either side, every second or third day, going and returning with the escort which accompanies the daily convoy. By this means the line is kept constantly in a state of repair, and small defects, such as crooked poles or wire too low, are at once remedied.

Next, as regards the "system of working":

There are two ways of looking at a field telegraph line: first, from a military point of view; secondly, from what may be called a general one. The greatest value of a military telegraph is the use it can be put to to warn a station of an impending attack, to arrange movements of troops, convoys, etc., and for this purpose it is absolutely necessary to be able to get any station at any hour day or night. On the other hand, its general importance in carrying long messages from headquarters to the base of operations, concerning the conduct of the campaign, commissariat and transport arrangements, besides news telegrams for the Press, cannot be overlooked. No doubt the proper plan is to have two wires, one working from station to station only, and the second a through wire. But this may not be possible, and the point is how to make the best use of one wire. On the Peshawur to Kabul line the traffic was so heavy that it was found necessary

to erect a second wire from Peshawur to Jelalabad, half-way; and even then the line could only just carry the traffic without serious delay. On a single wire line exceeding a certain length, say, 80 miles, on which there are more than about six offices, the best plan seems to be to divide the offices into classes, and not to allow them all to occupy the line equally, for the terminal and certain intermediate offices will always have most work.

On the Peshawur-Kabul line the offices were divided as follows:

(1.) Offices of observation.

(2.) „ doing limited duty, called second-class offices.

(3.) „ always open, called first-class offices.

As remarked in a preceding paragraph, for the speedy repair of the line in case of interruption, it is necessary to have offices every 10 or 12 miles; but it is not necessary they should be allowed to send and receive messages, except in cases of great emergency. These offices, which in Afghanistan were kept only for the sake of keeping up the communication, were called *offices of observation.* Only when the line was interrupted on either side did an *office of observation* keep open. In this case the signaller received all messages, and forwarded them across the break by the heliograph or best means available. *Second-class offices* were those to which certain hours were allowed for the use of the wire. Their instructions were to come in the first thing in the morning, from 10 to 11 a.m., from 3 to 4 p.m., and the last thing at night. In case of any expected attack or disturbance, particular offices were ordered to come in more frequently. *First-class offices* were the terminal offices, and two or three important intermediate ones, which were always open day and night.

At *first-class offices* the sounder with Siemens's relay, before described, was the instrument used. At each *second-class office* two direct working sounders were used, while at *offices of observation* one sounder and a galvanometer were found sufficient.

The *second-class offices* only worked with the station on either side of them, and with the nearest *first-class office.* No matter for whom their message, they gave it to the nearest *first-class office* (unless for the next station), and the first-class office forwarded it on; the object in view throughout being to allow

the *through* work, the most important in one sense, to be got off, and at the same time to give each small office a fair chance of getting off its messages.

It must be borne in mind that the telegraph was supplemented by the heliograph, and every station had its heliographic communication with the next during the day, when not clouded over.

Orders regarding the use of the field telegraph were issued from time to time by the lieutenant-general commanding, as necessity arose. These orders, slightly modified, if necessary, to suit altered circumstances, would probably be found effective in any future campaign. It is undesirable to put more restrictions on the use of the field telegraph than are absolutely necessary, but at the same time a single-wire line can only carry a certain number of messages, and, besides, the signallers should be guarded from overwork.

For a military telegraph, military signallers are in every way preferable to civilians. In the Afghan campaign, with two or three exceptions, the signallers were all soldiers, and may be said to have proved a great success. The late General Robinson, Director-General of Telegraphs, introduced the system of training soldiers at telegraph stations throughout India, and since the introduction of the system a large number of men have been trained and employed in telegraph offices. Had it not been for this, it is difficult to see how the Indian Telegraph Department would have been able to meet the demand for signallers in Afghanistan. As it was, every available qualified man, even from regiments on service, had to be taken.

The standard of qualification for a military signaller is to be able to read correctly by sound at the minimum rate of 12 five-letter words per minute, and to send 15 words; to know how to make up a Minotto battery, and to understand the ordinary connecting up of instruments according to the method in use in the Indian Telegraph Department. When he can do this he is qualified to work in an office, and receive remuneration at the rate of four annas, or about sixpence, a message up to a maximum of 25 rupees (about £2) a month. Military signallers when on field service in India

are no longer paid by the message, but receive 12 annas, or 1s. 6d., a day, and 8 annas, or 1s., a day more if in charge of an office. At this rate of pay there is no difficulty in getting men to learn; on the contrary, the telegraph service is becoming very popular with the British soldier.

It seems desirable that in future a certain number of men per regiment should not only be trained, but kept in practice. It will probably not be possible for any civil administration to employ *all* the trained soldiers in their offices, and rules should therefore be made to ensure each trained man getting a fair share of what employment can be given, and to prevent one man, because he is a specially good signaller, getting the whole. It is no use training a man unless you keep him in practice, and every trained man ought to have so many months' work yearly in an office in his due turn.

Another point is, that for field service the signallers should never be taken from regiments actually in the field. Commanding officers very naturally prefer their regiments to go on service full strength, and object to having men detached for telegraph duty. If the system above recommended, of having so many men per regiment trained, were carried out, there would be no necessity to take men from regiments in the field.

Each military signaller should, on qualfying, be given a book in which all his telegraph service should be entered. He should be re-examined from time to time in signalling, and the rate at which he can receive and send should be noted in his book. The book should show all payments and advances made him, so that, when he is transferred, the officer to whom he goes will have no difficulty in arranging his pay. Every signaller, before going on field service, should be paid up to date, and have all advances adjusted before he crosses the frontier. Only good signallers and men of good character should be selected for field service.

There is another point which in practice crops up. It sometimes happens that a *private*, being a smart man and good signaller, is placed in charge of an office, with a *corporal* or *sergeant* under him. This is subversive of all military discipline, but it is difficult to avoid, because the charge carries an extra

allowance with it, which should be given to the man who is best qualified for charge of the office. If some plan could be laid down for giving honorary rank to a deserving man, it would probably get over the difficulty. In the meantime, no corporal or sergeant, unless he is fit for charge of an office, should be sent to the front.

It seems a pity that there should not be a more intimate relation between the army signallers who work the heliograph, flags, etc., and the field telegraph signallers. The Morse alphabet is used by both in signalling, so that up to a certain point the system of training is the same. There is no reason why a man should not qualify for both. The telegraph service is better paid, and is preferred by the men, involving, as it does, less exposure. The amalgamation of the two systems is not advocated, but there is no reason why they should not draw their signallers from a common source, and so give more scope for the advancement of deserving men. As it is, the field telegraph and heliograph are bound to work together, the heliograph taking the work in advance of the telegraph, and also the work between stations which can communicate direct by this means, in order to save the wire from overwork.

The PRESIDENT: All members present will, I am sure, agree with me as to the interest and importance of the communication that has been read to us. What is required to be done in such cases as that with which Mr. Luke had to deal is very obvious, but it is not equally obvious how to do it. Mr. Luke has made very definite and precise recommendations and directions as to the method of carrying out military telegraphs, and I have no doubt many interesting suggestions and criticisms will be offered by those who are familiar with the work, and who are now present.

Major-General F. R. MAUNSELL (Royal Bengal Engineers): I heartily corroborate Mr. Luke's statement as to the cordiality between him and Sappers. The Colonel's remarks were due to a note of mine, describing the difficulties under which the military telegraph laboured in India, for want of a definite organisation for the field—the various experiences of the able officers of the civil telegraph department on the expeditions in and from India,

and the organisation each time arrived at, being thrown to the winds, to be begun anew on the next occasion, with a repetition of the same style of anxieties and obstacles. The organisation of such an element under military officers and soldiers, with time and the duty to arrange all during peace, on the same general basis as the civil lines, based on the experiences gained on these civil-military lines, and working with that department, had been on the *tapis* for years, struggling against active and passive obstacles of all sorts (although set agoing by the Secretary of State), and for the first time had been able to obtain employment in the field in Afghanistan; and, this being a most successful experience, I greatly regretted that the employment of this element, working hand-in-hand with the civil line, had been ignored by the heads of the civil or public works department of India. I considered that the obstacles to efficiency in these things was due to the tendency we too often indulge in of trusting to our wits and general experience, to our money and power, for improvising on emergencies, and to be satisfied with what has generally answered our former wants; that the advocacy of this efficiency was not the slightest reflection on the admirable work of the civil lines, although this has been an objection raised by those who do not appreciate the advantages and real economy of completer organisation; but that, on the contrary, such work furnished the very examples from which the advantages of organisation were noticeable! The obstructions by officials and departments, and the difficulties and misunderstandings necessarily arising therefrom, were in no way the fault of Messrs. Luke, Pitman, etc., who always worked on intimate terms of cordial friendship with the Engineers, each branch being always ready to aid the other.

Colonel Woodthorpe, R.E.: I have not much to say, as I am not an electrician; but I would like to mention one omission in Mr. Luke's category of the uses of the telegraph in the field, viz., its value in determining the longitude of a place. I was on the survey in Afghanistan, and had expected that determination of longitude by means of the telegraph would often be necessary. However, the country turned out to be much easier than we had expected, consisting of a series of open plateaux surrounded by

bare hills, high or low, from which good trigonometrical observations could be obtained. Few countries, I imagine, could be found so favourable in every way, either for the purposes of visual signalling or surveying. We were able to check the work frequently by bearings to fixed points; but if we had had to carry on long-route surveys in an easterly or westerly direction without any other checks, the telegraph would have been invaluable, as affording the means of obtaining our longitude by comparison of our time with that of some fixed trigonometrical station in India. Time would seldom have allowed of astronomical observations for longitude. Rain and haze did not often interfere with our operations. Major Wynne, who was in charge of the signallers in Kuram Valley, and who is now in charge of visual signalling at the Cape, writes to me that the country there is topographically almost as favourable to his work as Afghanistan, but rain greatly interferes with him; and he complains of want of enterprise on the part of the Boers, who will not go for the telegraph wires and give his signallers a chance.

Major ARMSTRONG, R.E.: India is such a very special country that, from want of personal Indian experience, I leave the discussion of the details of these operations chiefly to others who are acquainted with the country.

Major-General Maunsell has already mentioned the main point that occurred to me on hearing Mr. Luke's paper read, viz., the necessity for organisation of the field telegraphs of India.

A line advancing 3 miles a day, and worked by operators reading (nominally) 12 words a minute, cannot be considered as an adequate preparation for a struggle of such magnitude as we may be involved in at any time, and in which defective field telegraph organisation may be the weak link where a break occurs and the whole is lost. It is therefore to be hoped that the experience of the present and other campaigns will be utilised in the preparation of a definite organisation.

There is still much room for improvement in batteries which have been used for field telegraphy; and I quite concur with Mr. Luke in considering the batteries he mentioned as used in the ordinary telegraphic work of the country to be ill adapted for

field use: the gravity Daniell is manifestly not a good battery to carry about, and the Leclanché will not stand hard work.

The requirements of a really good field telegraph battery are—(1stly) it should not be readily damaged by carriage; (2ndly) it should be of moderately low resistance, so as to work through badly insulated lines; (3rdly) it should be constant and lasting—that is to say, its electro-motive force should not suffer serious deterioration either during hard work or while at rest; and (4thly) it should be compact, for the sake of portability.

Mr. E. C. Pitman, C.I.E.: Mention has been made during the evening of the military signallers employed in Afghanistan, and Mr. Luke said that he always found them exceedingly useful and well trained men.

I willingly admit their utility, but they were as a rule anything but well trained men. Their highest rate of signalling was twelve words per minute, and some of them, when they first joined me in the field, could only read by sound four or five words per minute. Under such circumstances it was most difficult to get through the work required of the telegraph department. The men did their work as well as they could, and, after some practice, we got them to send off their messages in something like decent time. The men were not so much to blame as the system under which they were trained. Anyhow, the delays at first were most serious, and at one time a break-down seemed inevitable.

The traffic at the commencement of the campaign was very heavy, and at Quetta the messages of one day had often to stand over till the following morning before they could be despatched, notwithstanding that the line was working throughout the night.

In Southern Afghanistan, the Minotto was the only kind of battery used. These batteries were packed in boxes of ten cells, in two rows of five; each cell had a separate compartment, and was surrounded with thick felt, and the ten zincs were kept in a compartment at the end of the box. When it was required to open an office, all that was necessary was to make the camels with the signal office equipments lie down; as many battery boxes as were required were taken off and opened; a little water was

poured into each cell; the zincs were placed on the sawdust of the cells and connected, and the battery was ready for immediate use.

In the Lushai campaign of 1871-72, I used both Minotto and Marié Davy batteries. The latter are most convenient and useful little batteries: they were packed in boxes about 15 inches long and 12 inches broad, and they did all that was required of them.

I must say that I disagree with Mr. Luke as to insulators not being required for field telegraphs. They can undoubtedly be dispensed with to a great extent in a dry country like Afghanistan, where there is no rainfall for many months, but if you go into a country like the Malay Peninsula, Ashantee, Lushai, or Bhootan, the dew and moisture are so excessive that the lines will not work until the sun is well up and the fog dispersed.

In the Lushai campaign we put up very few insulators at first, but the insulation was so low, owing to the heavy dews and the numerous tree contacts, that we had to increase the number from 15 or 20 insulators per mile to something like 30 or 40 in those places where the line ran through forest or grass jungle. After that it worked very fairly both by night and day.

Now as to interruptions. On the Khyber Line, the wire was cut, I think, 98 times in the first campaign and 50 times in the second; and I should like to ask Mr. Luke the cause of these constant interruptions, and if he was well supported by the military and political authorities, and if the persons who cut the line were punished. When the Bolan Pass line was first erected in 1877, the adjacent tribes cut the wire on several occasions, stole large quantities of it, and even pulled down the heavy iron standards and carried them off to a considerable distance. This was soon put a stop to by making a severe example of any one caught interfering with the line; and all through the recent operations, extending from the end of 1878 to the present time, the line through the Bolan Pass was hardly ever, if at all, wilfully damaged.

In Southern Afghanistan it was the same. The line, which was constructed of light bamboos and a wire weighing 300 pounds to the mile, was cut as it was approaching completion, a few miles from Kandahar, and some of our men were badly wounded. Several of the marauders were captured; some were shot during

the affray; two others were tried and hung; while the remainder were fined, and their village destroyed. In another instance a boy was flogged for cutting the line.

These punishments had the desired effect, for during the next nine months, that is, from April to December, 1879, there was hardly another case of injury to the line in a distance of 140 miles; and I must again express my best thanks to General Stewart, and to another officer who is well known to telegraphists, Colonel St. John, R.E., the political officer at Kandahar, for the ready and efficient help they always gave me on all occasions.

I did not notch any of the poles on the Quetta-Kandahar line. As a rule the 300-lb. line wire was fixed to the sapling or bamboo by a piece of 600-lb. iron wire, a short piece of which was twisted round the top of the pole; one end was turned and formed into a hook on which the line wire was placed, and bound to it with a piece of No. 15 B.W.G. wire.

A question has been asked relative to the collection of material. I have had on one or two occasions to execute heavy works, and the issue of stores was invariably left to our own store department. The arrangement for the provision of the stores sent to Afghanistan devolved on Mr. Brooke, the officiating director of construction, and their actual issue on Major Eckford, R.E., the superintendent of the Calcutta store-yard, and I must say that the equipments were most complete. The stores were well packed, and, if anything was wanted quickly, it was despatched at once on the receipt of a telegram asking for it. Nothing was forgotten, and, had it not been for the forethought and exertions of these two officers, great delays must have ensued.

I should like to mention that the Kandahar military telegraph was made into a commercial line, from the 1st June, 1879, and it remained so until it was destroyed last autumn. As a commercial line it was thrown open to the public without restriction, and the remittances to India from the Kandahar office alone amounted to over £10,000; and I think the total receipts of the line almost covered its cost.

It is a matter of deep regret to me that, owing to the evacuation of Kandahar, the line thither should recently have been

pulled down. It now only reaches to Chaman, 77 miles east of Kandahar, and about 63 miles west of Quetta. I regret its demolition all the more, because I think it will again be required, and that we shall have to go back to Kandahar before very long. (Loud applause). Having gone back there, it will be necessary to establish a political agent at Herat, but this will not be done unless we have a telegraph there, and there is no reason whatever why a telegraph should not be laid between Kandahar and Herat. Having got a line to Herat, you are only 220 miles from Meshed, to which point the Persian Government have a telegraph at this moment. To Teheran there is an excellent first-class line from Europe, and with very little outlay the existing line between Teheran and Meshed might be put into good working order and be maintained without difficulty. If the Persian Government can maintain a line between Teheran and Meshed, I do not see why a line should not be maintained for the comparatively short distance of 220 miles between Herat and Meshed. If the Turkomans can be held in check and prevented damaging the line on one section, can they not be checked on the other? The total length of line required to connect the Indian and Persian telegraph systems is about 600 miles, viz., 375 miles from Kandahar to Herat, and 220 from Herat to Meshed; and this link would complete a land line of telegraph between India and England. The existence of such a line would enable the present rates to be reduced very considerably, and, instead of having the present high charge (not unnecessarily high considering the cost of cables) of something like five shillings per word, we should have a rate of perhaps one shilling per word. By taking the chiefs of the native tribes inhabiting the districts through which the line would pass, into our employ, and making them responsible for the line, as was done so successfully in Beluchistan and Southern Afghanistan, freedom from malicious interference would be obtained, and the line would be kept up without much danger or difficulty.

The poles used on the Kandahar line were partly teak saplings sent up from Bombay, bamboos bought at Kurrachee, and 33 miles of poplar and willow poles bought at Kandahar; we also had

50 plane (*Platanus orientalis*) poles for the Kandahar end of the line.

The teak saplings averaged in length 17 ft. 9 in.; the average diameter at the base was 3¾ in., and the average at the top 1½ in.

The plane tree poles purchased at Kandahar were rather more substantial, and were used when passing through the villages immediately outside Kandahar town. Their average length was 18 ft.; diameter at butt, 5 in.; diameter at the top, 2 in. They cost four shillings each, and weighed about 50 lbs.

The poplar and willow poles bought in Kandahar ran from 13 ft. to 18 ft., and only cost one shilling each.

No poles were procurable in the country at any other place except Kandahar, and none are available between Kandahar and Herat, though I believe a certain number could be obtained in the villages round Herat.

The bamboos were about 22 ft. high, and were chiefly used for road crossings. Where they were employed on a straight alignment, they were put up about 90 or 100 yards apart; the distance between the teak saplings, willow, and poplar poles varying from 70 to 110 yards.

With regard to transport. We employed camels, mules, and ponies. The Government regulation weight for a camel was 320 pounds, and for a mule or pony 160 pounds; but the camels could rarely carry more than 250 pounds. The animals were very sick and weakly from the hardships they suffered during the winter of 1878-79, from exposure, and want of proper food and clothing.

I think you will like to hear that during the siege of Kandahar last year, throughout the whole of that anxious and critical period, the telegraph workmen, who were mostly Afghans, remained staunch and faithful, although they belonged to the disturbed districts. Day after day these men, who were unable to go out on the line and repair it, were employed in knocking down walls and houses, and any other buildings likely to impede the fire of our guns, putting up wire entanglements, and keeping up telegraphic communication between the most important points of the besieged town. Mr. Boteler, the officer in charge of the telegraph at Kandahar, was not only engaged by day as an assistant field engineer,

but at night he had to go on duty as an officer attached to a native regiment. I mention this, and I do so with pride, because it ought to be made known. This is a most suitable occasion for mentioning such services, and I am sure you will all be interested in, and be glad to hear of such matters.

Professor W. E. AYRTON: As to the question of batteries, might I ask if the Minotto used in the Indian campaign was of the ordinary Minotto form, or some diminutive cell specially designed for the occasion?

Mr. PITMAN: Yes; the ordinary form.

Mr. PITMAN: In reply to Col. Webber, who enquired as to the failure of the line in wet weather, I would say that in March, 1879, we had some very severe rain and snow, and on those occasions the communications failed entirely.

Col. WEBBER: From want of insulation?

Mr. PITMAN: Yes; and at other times, when a slight shower was passing over any portion of the line, the communication became so bad that the small intermediate offices had to open out, and messages were then repeated, always once, and sometimes twice, between Kandahar and Quetta, a distance of only 140 miles, thereby very considerably delaying them. No such delay would have occurred had the line been insulated.

As to friction with the military telegraph, we had none at all. They were unable to get all their stores to the front on account of want of transport, and the Sapper telegraph company was generally employed making roads. At Kandahar the Sappers had a short line, about 1½ miles long, connecting the headquarters and Engineers' camp with the Goverment telegraph office in the Kandahar citadel, and we worked most cordially with them.

With regard to the civil employés of the Government telegraph department being under military law, I certainly understood that they were amenable to military discipline, and should say most decidedly that Sir Donald Stewart was of the same opinion. I myself was immediately under the orders of the General commanding the force, and I had written instructions to this effect from the Adjutant-General.

I took my orders regarding the route for the line, quarters,

position of offices, etc., etc., from the Assistant-Quartermaster-General on the staff of General Stewart, and not from my own department, except on matters of detail; and when any of my civilian workmen misconducted themselves, they were punished by the Provost-Marshal.

On the other hand, the only people who escaped punishment for offences in the field were the military signallers (the Indian Telegraph Act neither applied to them nor to offences committed out of India); and the military authorities, as represented by the Judge-Advocate-General's department, expressed their inability to punish soldiers for what they termed civil offences. These civil offences, mind you, included wilfully delaying work, and refusing to signal messages—offences serious enough under ordinary circumstances, but much more so on active service, when delays to telegrams might cause very grave results.

Colonel Webber may rest assured that our civilian employés were well provided for in the way of punishment, and that the only persons who did not suffer for their misdeeds were the military signallers, who could not be brought within the scope of either military or civil law.

A hearty vote of thanks was then passed to Mr. Luke for his paper.

Lieut.-Col. MALCOLM, R.E., C.B.: I should just like to utter one little word of protest. The paper began by a quotation from Mr. Preece, who is used as an authority with reference to what is called *his* division of the field telegraph service into four parts. I have no objection to raise against that at all, except that it was officially recognised in the army long before Mr. Preece's lecture was delivered; but I hope that the lecture this evening will not be taken as an authority for doing away with any of those four divisions, because Afghanistan happens to be peculiarly adapted for visual signalling. I am certain that it would be most dangerous to abolish the flying, or field line.

There are many other countries besides Afghanistan with which I am afraid we may have some day or other to go to war, which will not lend themselves so freely to visual signalling, and where also, as has been pointed out, insulators will, under

existing circumstances, be a necessity. In Ashantee, for example, it is constantly wet, and, should we again go to war with that country, a flying telegraph would be an indispensable portion of the equipment.

On behalf of soldier officers, I would like also to add that we cannot always do just what we like even in keeping up little bodies of trained men in our regiments; and I do not think the proposal to draw the signalling staff from regiments not engaged in active operations would always work well.

In conclusion, I am delighted to hear that soldiers and civilians worked so well together at the front.

Lieut.-Col. C. E. WEBBER, R.E.: Mr. President and gentlemen,—I have on former occasions addressed the Society on this subject, and I only wish to take up your time to-night by a few words on the paper of Mr. Luke, which, I must say, has a true practical ring about it. It bears evidence to the magnificent work of which he was the superintendent during the Afghan campaign. I fully agree with him (and have done so for a long time) in his condemnation of the use of a ground cable as a means by which you can ensure the transmission of messages during any considerable period. He says the ground line, thirty miles long, "never lived for an hour:" I only wonder that nobody, so far as I can remember, has put it so strongly before.

On the other hand, he says "it is extremely probable that an army would traverse a country in which they could always procure suitable poles;" but Major Hamilton, R.E., who commands the field telegraph at Aldershot, can tell you that he worked for several months in Natal and Zululand, where not only was a suitable pole unprocurable, but there were no poles at all; and every pole had to be carried from the sea-shore to the places where required. The poles used in that case were bamboos from India and Brazil. A great number, some 4,000, came from Madras; and the Emperor of Brazil made the British Government a present of between 2,000 and 3,000, for which, I am sorry to say, he never received any thanks, and which were sold eventually for about sixpence apiece. I do not agree with Mr. Luke that the "joint" is the weakest part of a pole. Poles which have to be carried (as he told us) by mules

and camels must not have longer sections than 7 or 8 feet. I have made several experiments in this direction myself, and I found that not only could a connecting steel tube be used, like the joint of a fishing rod, most satisfactorily, but also that a splice-joint bound by wire, in the hands of a man who understood how to use wire, was quite safe. At the present moment, at Woolwich Arsenal, there are, I believe, between 2,000 and 3,000 of such poles, ready at any moment to be taken where they are required.

Mr. Luke tells us also that insulators are entirely out of place in the field. Well, I think he will willingly, after what Mr. Pitman has said, withdraw partially that opinion, because he must be aware that campaigns will be conducted in many countries where the moisture is great, and where the rainfall would absolutely destroy the insulation, and in fact where the insulation would be very partial with the best insulators. I know that, until Andrew's double-shed insulators were introduced on the lines through Egypt, in the heavy rains (which, however, rarely occurred) insulation was absolutely destroyed. Mr. Luke has not told us (neither has Mr. Pitman) whether during the campaign they had any heavy falls of rain, and, if so, what they did during those periods.

As regards the wire, I think that they hit upon an admirable size in their No. 12 Indian gauge, which appears to be the same as No. 11 (or a little larger than No. 11) Birmingham gauge. We have found, not only in this country in Government telegraphs, but also in the field in other places, that No. 11 wire is, after all, the most handy, the most economical, and the best wire for your main military lines. Some of you will remember that Lieut. Jekyll, R.E., on his return from Ashantee, read us an interesting paper on the military telegraphs in that country, in which he told us of the great advantages No. 11 wire offered for military lines.

Curiously enough, what so often happens in other countries, occurred in India—the war was entered upon without apparently any preparation being made for the military telegraph, and this, I think, Major-General Maunsell has also led us to believe.

The military material which existed in the country was probably at Roorkee, and its maintenance had been more or less neglected for the previous ten years, and there was, I imagine,

only enough to take the field to construct a very short line. Mr. Luke has described how it was used in the first advance towards Candahar in front of his line, and afterwards taken down and re-erected further on. A certain amount of dissatisfaction apparently, and most probably, arose under these circumstances: in fact I find, from a report made at the time, that the Departmental and Sapper lines were rather in the position of jostling each other all along the road. When an advance was made it was the same thing over again, though I believe such was not the intention of the General Commanding. "The Sappers made a line directly the troops moved, and with a rapidity which reflects credit on them; the offices got to the front at the same time as the advance guard, and, in fact, at one time the work was ahead of the advance guard."

Mr. Luke's remarks show the necessity for organisation and preparation in time of peace. The nucleus of military telegraph at Roorkee only had an insignificant quantity of material, and, of course, as a matter of common-sense, the National Telegraph of India was called upon to take the field and construct the line (the military telegraph going in advance), because they had an unlimited quantity of material. The trained staff were men of calibre, such as those gentlemen from India who have addressed you to-night, and they were the proper people to do it.

When a war breaks out you use the best and readiest means at hand, and there is no question whatever, in the mind of any one, of that being the right thing to do. If the military telegraph, or field telegraph, whether it be called military or not, had been organised before, and organised so that there should be harmonious joint working with the state telegraphs, and the nucleus (small or large) of military telegraph had been used for a special purpose, none of the friction which perhaps rose at first need have occurred. We were told, on one side, that there was considerable loss in consequence of the lines not being so well to the front as they should have been; on the other hand, we have heard this evening that in the first instance the military telegraph was full up with the advance guard. This conflict of statement also points to the previous organisation which Major Armstrong considers so necessary.

I am sure there could have been no wiser step taken than the organisation which has been established in this country, and which occupied eight or nine years of hard work to enable it to come to success, and enable it to do what it is intended to do. We can place a considerable military telegraph force in the field, and there is every reason why that should be followed up by a considerable civil telegraph force: there is every reason in favour of it, and, had it not been for the want of the encouragement to the formation of a volunteer telegraph force in the Post Office, it would have come into existence long ago.

Mr. Luke has not told us whether the civil officials of the telegraph department in India, when they took the field, were under the Indian Mutiny Act, when beyond the Indian frontier, or not. That to a soldier seems a very necessary thing, because there can be no perfect discipline unless every one employed in the field is under the same law.

Mr. Luke's paper is one of the first on the construction and progress of telegraphy in an uncivilised country that has been read here. I hope that it will show to our comrades and fellow-members of the Society in India the interest that is shown by it in all that comes to us, especially from the large body of officers, fellow-telegraphists, and men in the service of the State Telegraphs who have been working in the defence of our great Indian dependency and Empire.

Mr. JOSEPHS: I am afraid, sir, that I can add very little to what Mr. Luke has already read to us in his very able paper. I carried out my portion of the work very much on the same lines as he did. I need not tell you that I went up a hill 100 feet higher, and had to climb over a little worse country than Mr. Luke.

There is one feature connected with this military telegraphy to which I would like to call attention, and that is a question of organisation.

I think that the erection of flying lines in the field might be much accelerated if a certain number of men in every regiment were instructed both in the erection of lines and trained as signallers, they being always attached to and remaining with their regiments. If that were so, as the army advanced, the construction

of the line could be done without any difficulty, no special detachment of parties or any other arrangement being necessary. Each brigade as it advanced would take up its own piece of work, and a stronger and better line could thus, in my opinion, be erected than under present circumstances.

Major HAMILTON, R.E.: Colonel Webber has kindly mentioned my name as being connected with the military telegraph. I am in command of the telegraph troop at Aldershot, and I should like to make one or two remarks.

First, as to insulated wire not having been any good. As far as my experience goes, I found that the insulated wire taken to Natal for the Zulu war worked extremely well. We did not lay it down very close to the roads, and we had very few interruptions along it. The worst interruptions that occurred were caused by grass fires, by which the insulation was burned off, and, consequently, the current went to ground.

Another point, one which I thought would have been mentioned by Major Armstrong, is the rapidity with which a field telegraph can be run by laying a bare wire along the ground. Some recent experiments have been tried at Chatham and at Aldershot, which show that by using the Theiler sounder (which gives a musical note instead of a click) at one end, and a telephone at the other end, it is possible to work with a bare wire of some length over any kind of ground, whether dry or wet. We laid some bare wire along marshy ground, and signals came out as clear as possible, enabling signallers to read up to 30 words a minute. In order to test the system a little further, we took some 30 yards of the uncovered line wire and dropped it into the canal, when signals came out as distinctly as before. A further test was, to cut the wire in the canal, when the ends flew back from each other about 9 or 10 feet, and the signals were perfectly distinct, although not so loud. I may also observe that we poled a large quantity of this line, and, whilst the operation of raising this wire on poles was going on, the signals continued uninterrupted. I have seen a little of this done before in Natal and in the Transvaal (Col. Webber having kindly brought us out some telephones); and on one occasion, when we had laid some bare wire along the ground, we were getting

signals on the sounder while the ground was dry, but in the early morning, when there was dew, the sounder would not work. However, when we put a telephone on at the receiving station, instead of a sounder, messages were received quite uninterruptedly, the click of the sounder at the sending station being quite distinct; but that is nothing whatever in comparison with the Theiler giving its vibrating currents which can be heard through any resistance, and with any amount of leak that it is possible to imagine.

Mr. W. H. PREECE: I think I can add a little to the experience and the application of telegraphy, by referring to the very last telegraph that has been applied to military purposes. Of all the telegraphs that have been sent out from this country, this has perhaps been the most successful—I mean the most successful in that, while it never was put up, it succeeded in doing all that it was intended to do, and a great deal more. If you remember, a short time ago an alarm came to this country of unexpected troubles with Ashantee. One Friday morning the Crown agents for the Colonies communicated with me, and asked me if it was possible to send out to the Gold Coast 20 miles of telegraph fully equipped with stores and men and telegraphists, and to be on board ship at Plymouth on Tuesday morning. From Friday to Tuesday was rather a short time to send out a fully equipped telegraph, but with the assistance of the Royal Engineers, who are attached to the Post Office telegraphs, it was done—it was done on Monday afternoon; and in less than a fortnight this telegraph was at the Gold Coast, and the effect of it was to prevent the outbreak of war which, without it, would probably have taken place. Lord Kimberley reports that, the moment the Ashantee king heard that the telegraph had arrived, he at once proclaimed peace.

I remember, in my young days, that we cricketers used to have a great contempt for what we called fireside-cricketers; and I am afraid that some of my military friends have a certain amount of contempt for fireside military lecturers and electricians like myself. I have criticised their performances, and have called into question their acts, and have on many an occasion preached to them one sermon, and that sermon was upon the text of "Use the sounder." And if one part of Mr. Luke's paper has afforded me more gratifi-

cation than another, it is the fact that in India they have found, by actual demonstration, that the practical instrument to use for such purposes is the sounder; and we know from experience in the Post Office, that we get more work, more accuracy, certainly greater simplicity, and more perfection altogether, by the use of the sounder than by any other instrument, except perhaps, for certain purposes, the telephone. I was very glad indeed to hear that the experiments with telephones which were being carried out at Chatham and Aldershot were so promising of success. I may say that in the telegraph equipment sent to the Gold Coast telephones were included: whether they have proved successful or not, I have not yet heard.

I was rather surprised to hear from Col. Webber that attempts have been made for the last ten years to introduce a volunteer telegraph service, and that the object has failed, owing to the opposition of the Post Office officials.

Colonel Webber: I did not intend to say the Post Office, I meant the War Department.

Mr. W. H. Preece (continuing): I was going to say that, ten years ago, I undertook to take charge of this volunteer telegraph corps. I was temporarily gazetted as a captain, but I have never yet received my commission, owing, I believe, to the opposition of the War Department.

I could say a good deal more on this point, but the closing hour has arrived, and I can only express my very great gratification that my old friend Mr. Luke has come forward to assist us with that which we want so much—experience and practice.

Mr. S. P. V. Luke, C.I.E., in reply, said: A word upon the remark made by Colonel Webber as to the existence of friction between the civil department and the Sappers in the first campaign. As far as I know, there was no friction whatever, and I think General Maunsell will bear me out in saying that, and that we always worked together most satisfactorily without any friction. Some of the work was done twice over, but that was simply on account of inexperience in working together.

As to whether the civil officers were under the Mutiny Act or not, I cannot say, but as far as I know we were on the same

footing as every one else on active service across the frontier. We were transferred to the military department altogether, and were placed under the orders of the Lieut.-General Commanding.

The PRESIDENT: Before the meeting dissolves I have a matter of some importance to communicate, and that is in connection with the Exhibition of Electrical Apparatus in Paris, which you are all aware is to be held this summer. It has been decided by the Council to invite members to attend an extra meeting of this Society in Paris during the Electrical Congress which will take place in September and October.

What is at present proposed is that the Society should, according to the amount of business forthcoming, remain in session for one or more days in the latter part of September. But detailed arrangements will be made and announced to members as soon as possible.

A ballot for new members then took place, at which the following were elected, and the ordinary meeting was adjourned until the 10th November, 1881:—

As Foreign Members:

Antoine Bréguet.
Edouard Hospitalier.

As Members:

Alfred Dillon Bell.
Charles Curtoys.
Warren De la Rue, D.C.L., F.R.S.
John Rapieff.

As Associates:

Shelford Bidwell.
William Blandford.
Herman Erichsen.
John J. Gilbert.
James Haig D. Jones.
H. C. Wilson.

As Student:

Edwin M. Brooks.

ORIGINAL COMMUNICATIONS.

MAGNETO-ELECTRIC CURRENT GENERATORS.

By Oliver Heaviside, Associate.

Perhaps the simplest specimen of a magneto-electric current generator is a coil rotating with uniform velocity in a uniform field of magnetic force. To get the greatest effect, the axis of rotation must be at right angles to the lines of force. The variation of the amount of induction through the coil induces a simple harmonic E.M.F. in it, and the result, when the initial effect has subsided, is a simple harmonic current. But the phase of the current is behind that of the E.M.F., owing to the self-induction of the circuit, which also diminishes the amplitude of the current waves.

Otherwise, we may consider the current at any moment to be that due to the actual E.M.F. round the circuit at that moment, according to Ohm's law, remembering that the actual E.M.F. is the algebraical sum of the E.M.F. due to the motion, and that due to the variation of the current itself.

Symbolically, let M be the induction through the coil when its plane is at right angles to the lines of force of the external field, ωt the angle turned through from this plane at time t, the angular velocity of rotation being ω; then $M \omega \sin. \omega t$ is the impressed E.M.F. in the coil. And if R is its resistance, R_1 the external resistance, L the coefficient of self-induction of the coil, L_1 the external ditto, the equation of the current is

$$M \omega \sin. \omega t = (R + R_1)\gamma + (L + L_1)\dot{\gamma}$$

consequently

$$\gamma = \frac{M \omega \sin. \omega t}{(R+R_1)+(L+L_1)D} = \frac{R+R_1-(L+L_1)D}{(R+R_1)^2+(L+L_1)^2\omega^2} M \omega \sin. \omega t$$

$$= \frac{M \omega \sin. (\omega t - \theta)}{\sqrt{(R+R_1)^2+(L+L_1)^2\omega^2}}$$

where

$$\theta = \tan.^{-1} \frac{L+L_1}{R+R_1} \omega$$

The amplitude of the current is therefore

$$\frac{M \omega}{\{(R+R_1)^2+(L+L_1)^2 \omega^2\}^{\frac{1}{2}}}$$

and the angular displacement of the zero is θ.

The above is applicable, or nearly so, to any magnetic machine with a single coil: for example, a Siemens' armature revolving between the poles of powerful magnets, the effect of the iron armature being in the main simply to increase M and L.

By reversing the connections of the coil with the external circuit at the moments of zero current,—that is, something between 0 and $\frac{1}{4}$ revolution after zero E.M.F., according to the speed, etc.,—the external current is put into one direction; but since the current varies greatly in strength, the effective E.M.F. of the machine must be taken as the product of the mean external current into the resistance of the circuit, supposing, of course, that there are no other E.M.F.'s acting in the circuit than already considered.

If Γ is the mean external current,

$$\Gamma = \frac{2}{\pi} \frac{M \omega}{\{(R + R_1)^2 + (L + L_1)^2 \omega^2\}^{\frac{1}{2}}}$$

and the effective E.M.F. of the machine is

$$\Gamma (R + R_1)$$

The mean current increases directly as the speed at first, but afterwards more slowly, and its limiting strength is

$$\frac{2}{\pi} \cdot \frac{M}{L + L_1}$$

that is, the ratio of the mean amount of external induction through the coil to the self-induction of the circuit per unit current. (To bring into webers, multiply by 10.)

The theory of multiple coils is quite similar, and resembles that of galvanic cells joined up in series or in multiple arc. If any number n of similar coils rotate simultaneously in the same magnetic field, and are equally acted upon by varying induced

E.M.F.'s, all in the same phase, the coils may obviously be joined up all for "quantity" or "intensity" without any interference, a simple harmonic current resulting equivalent to that from a single coil with constants $n\,\text{M}$, $n\,\text{R}$ and $n\,\text{L}$ in the intensity case, and M, $\frac{\text{R}}{n}$ and $\frac{\text{L}}{n}$ in the other. And reversing the coils all at the same moment, the external current is put in one direction, as before, though of very varying strength.

With only one coil, we cannot get rid of this great variation, but using many coils we may reduce the variations as much as we please, multiplying their frequency at the same time, by making the phases of the induced E.M.F.'s differ by equal amounts and reversing every coil at the moment of zero current for that coil, supposing the others not to act. And, curiously enough, the resultant mean external current is not affected by the changes.

Thus, to fix ideas, suppose we have any number of coils arranged at equiangular intervals round a circle, and revolving together in a uniform field of force. It is unnecessary to specify any particular form of machine. The two coils at opposite ends of any diameter have exactly equal E.M.F.'s acting on them at any moment, so they may be joined together permanently, and treated as a unit in the arrangement. Thus we have, say, n pairs of coils, which have all equal simple harmonic E.M.F. acting on them, but at different times.

Joining them up in series by means of an n-fold commutator, which reverses the coils one after another in proper order, let M, R, and L be the constants used before, but now referring to a pair of coils, and suppose No. 1 pair to act alone,

We have

$$\text{M}\,\omega \sin.\, \omega t = (n\,\text{R} + \text{R}_1)\,\gamma + (n\,\text{L} + \text{L}_1)\,\dot{\gamma}$$

therefore

$$\gamma = \frac{\text{M}\,\omega \sin.\,(\omega t - \theta)}{\{(n\,\text{R} + \text{R}_1)^2 + (n\,\text{L} + \text{L}_1)^2\,\omega^2\}^{\frac{1}{2}}}$$

where

$$\theta = \tan.^{-1} \frac{n\,\text{L} + \text{L}_1}{n\,\text{R} + \text{R}_1}\,\omega$$

Here γ is the current in No. 1 pair, due to its own motion, but

as it is reversed at the moments of zero current for itself the mean external current due to pair No. 1 is

$$\frac{2}{\pi}\frac{\mathrm{M}\,\omega}{\left\{(n\,\mathrm{R}+\mathrm{R}_1)^2+(n\,\mathrm{L}+\mathrm{L}_1)^2\,\omega^2\right\}^{\frac{1}{2}}}$$

Now, letting all the coils work, and superimposing the currents, we have a mean external current Γ, where

$$\Gamma=\frac{2}{\pi}\frac{n\,\mathrm{M}\,\omega}{\left\{(n\,\mathrm{R}+\mathrm{R}_1)^2+(n\,\mathrm{L}+\mathrm{L}_1)^2\,\omega^2\right\}^{\frac{1}{2}}}$$

the same as from a single coil with constants $n\,\mathrm{M}$, $n\,\mathrm{R}$ and $n\,\mathrm{L}$, but with the difference of having many small variations from its mean strength in place of few large ones.

It is to be remarked here, that although every pair is successively reversed at the moments when its own current is zero, yet, since it has to carry the currents from the remaining $n-1$ pairs, this current is necessarily reversed suddenly: so that whilst externally we have a nearly steady current, and also in the coils in series, considered as a whole, yet a regular succession of abrupt reversals of current is going on all through the series.

So to minimise the unavoidable sparking, we should theoretically subdivide the coils as much as possible, thus making the inertia of the currents to be reversed as small as possible; or make the reversal a continuous operation, instead of intermittent.

Joining the circle of coils in one continuous series, and putting the external circuit on between opposite ends of the neutral diameter, gives an external current equivalent to that from a single coil, with constants $\frac{n}{2}\,\mathrm{M}$, $\frac{n}{4}\,\mathrm{R}$, $\frac{n}{4}\,\mathrm{L}$.

Now, arranging the commutator to connect the n pairs of coils for quantity, we have, considering the first pair alone to act,

$$\mathrm{M}\,\omega\,\sin.\,\omega\,t=\mathrm{R}\,\gamma+\mathrm{L}\,\dot{\gamma}+\mathrm{R}_1\,\gamma_1+\mathrm{L}_1\,\dot{\gamma}_1$$

and also

$$\mathrm{M}\,\omega\,\sin.\,\omega\,t=\mathrm{R}\,\gamma+\mathrm{L}\,\dot{\gamma}-\frac{1}{n-1}\,(\mathrm{R}\,\gamma_2+\mathrm{L}\,\dot{\gamma}_2)$$

where M, L, R, L_1, R_1 are as before, but now γ is the current in No. 1 pair, γ_1 the external current due to it, and γ_2 the total current in the remaining $n-1$ pairs due to No. 1.

Also, by continuity, $\gamma + \gamma_2 = \gamma_1$.
Therefore

$$\gamma_1 = \frac{M\,\omega\,\sin.\,\omega\,t}{(R + n\,R_1) + (L + n\,L_1)\,D};$$

$$\gamma_2 = -\,(n-1)\,\frac{R_1 + L_1\,D}{R + L\,D}\,\gamma_1\,; \quad \gamma = \gamma_1 - \gamma_2.$$

For the external current we have

$$\gamma_1 = \frac{M\,\omega\,\sin.\,(\omega\,t - \theta)}{\{(R + n\,R_1)^2 + (L + n\,L_1)^2\,\omega^2\}^{\frac{1}{2}}}$$

where

$$\theta = \tan.^{-1}\frac{L + n\,L_1}{R + n\,R_1}\,\omega$$

This is due to pair No. 1, supposing it not reversed. But reversing it at the proper times, we have a mean external current $= \frac{2}{\pi} \times$ amplitude of γ_1; and consequently n times as much when all the coils act, or

$$\Gamma = \frac{2}{\pi}\,\frac{M\,\omega}{\left\{\left(\frac{R}{n} + R_1\right)^2 + \left(\frac{L}{n} + L_1\right)^2\omega^2\right\}^{\frac{1}{2}}}$$

the same as from a single coil with constants M, $\frac{R}{n}$ and $\frac{L}{n}$, with of course the difference of being nearly steady.

For simplicity of expression we may replace a multiple coil machine by its equivalent single coil machine, say, with constants M, L, R, and with external current:—

$$\Gamma = \frac{2}{\pi}\,\frac{M\,\omega}{\{(R + R_1)^2 + (L + L_1)^2\,\omega^2\}^{\frac{1}{2}}}$$

Varying the size of the wire, since M varies as the number of turns, whilst R and L vary as its square, the following expression follows as the condition for maximum current with a given speed of rotation:—

$$R^2 + L^2\,\omega^2 = R_1^2 + L_1^2\,\omega^2$$

Neglecting the self-induction of the external wire, the resistance of the machine should be less than the external resistance, and more so with higher speeds.

Professors Ayrton and Perry advocate extremely high speeds. Only make the speed high enough, and R becomes very small. At

the same time a counterbalancing factor will come into play, namely L_1. Also at excessively high speeds the electrostatic capacity of the line and other things would need consideration, so that on the whole it is perhaps premature to say what the resistance of excessively high speed machines should be. But as the experiments mentioned in Mr. A. Siemens' paper show that the current is nearly proportional to the speed, it is possible that there is still left a wide margin for further increase.

NOTE.—Since the above was in print there has appeared in the *Electrician* (June 18, 1881, p. 70) an article on "The Theory of Alternating Current Machines," relating to M. Joubert's experiments and theoretical conclusions. Finding experimentally that the current from an alternating current machine could be represented exactly by the formula

$$I = \frac{e}{(R^2 + a^2)^{\frac{1}{2}}}$$

where I is the mean current, R the total resistance, a a constant proportional to the speed, and e a constant = quotient by $\sqrt{2}$ of the maximum E.M.F. of the machine with open circuit, measured by a Thomson portable electrometer, M. Joubert sought to justify it by theory, and gives the theory of a revolving coil, similar to that on the first page of the above paper. There is, however, this peculiarity, that although M. Joubert assumes the existence of a simple harmonic induced E.M.F., which implies a uniform external field of force, he only brings in the speed as a factor afterwards as an experimental result, whereas it is a necessary consequence of uniform speed in a uniform field; thus,

$$\text{E.M.F.} = -\frac{d}{dt} \, M \cos. \omega t = M \omega \sin. \omega t.$$

Also M. Joubert employs the numerical factor $\frac{1}{\sqrt{2}}$ instead of the $\frac{2}{\pi}$ which I have used.

PADDINGTON STATION,
June, 4th, 1881.

To the Chairman of the Editing Committee.

MY DEAR SIR,—Referring to Mr. A. J. S. Adams' paper on earth currents, read on the 10th of February last, and the discussion thereon, in which the question " of the effects of the flow of water against or under an insulated wire " were discussed, I gave on a previous occasion the results of some experiments I had tried; but, as they were not to my mind satisfactory, I had further tests made, and now give you the results of the same, which, in my opinion, quite disposes of the question, and gives an instructive result.

A No. 7 G.P. wire, 18 copper, was laid across the Thames at Reading. The current at this spot was running about 14 miles per hour, so the place was well selected for the trial.

Wire in the Water.

	Deflection.
Wrought iron earth plates at both ends	24°
Galvanised ,, ,, ,, ...	15°
Copper ,, ,, ...	Nil.

Wire out of the Water.

With the same earth plates, results were the same as above.

Again.

Wire in Water.

	Deflection.
With a wrought iron earth plate at one end and a galvanised ,, ,, at the other end	30°
Wrought iron at one end and copper at the other...	75°
Galvanised iron ,, ,, ,, ...	79°

Wire out of Water,

and with the same arrangements of earth plates, results the same as above.

A telephone was then put into the circuit, and a faint rumbling noise was heard, but this did not in the least interfere with the transmission of articulate sounds.

Thinking that as iron earth plates always produced a current, and that as the wire on which the foregoing experiments had been

made had been of copper, I had a No. 16 galvanised iron wire run up about 70 yards or so, well insulated, and, on trying this wire with the same earth plates as were used before, the following results were obtained, the same astatic galvanometer being used:—

	Deflection.
With copper earth plates at each end	Nil
Galvanised iron rods at each end	54°
Wrought iron rods at each end...	38°

These experiments conclusively show no electrical current is produced in the wire by a current of water against, over, or under it, and also clearly point to the fact that copper is the proper material to use for earth plates, whether iron or copper "line wires" are used. Those who are carefully watching the effects of earth currents should use no other, or they may get results from chemical action which may very easily mislead, and be mistaken for natural earth currents.

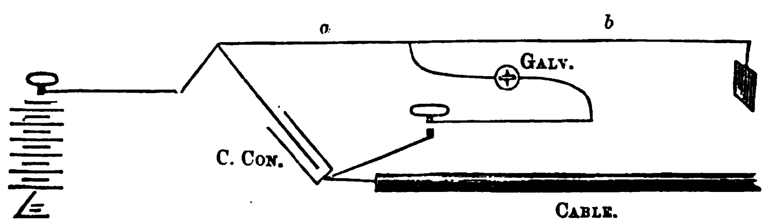
I am, my dear Sir,
Yours truly,
C. E. Spagnoletti.

Prof. Ayrton.

MEASUREMENT OF THE ELECTROSTATIC CAPACITY OF CABLES AND CONDENSERS.

By J. Gott, Member.

The following method of comparing the electrostatic capacity of condensers, or long cables, gives identical results with the valuable system proposed by Sir W. Thomson, and is sometimes more convenient, as a carefully insulated battery is not required:—



One end of a slide or adjustable resistance is connected to

earth, the other to a battery key and condenser (whose capacity in microfarads is known). To the opposite side of this condenser is connected the cable or condenser which we desire to measure.

A galvanometer or electrometer is connected to the sliding contact, and to a key which makes contact with the junction of the cable and standard condenser.

The battery is now to be applied, and after an interval of five seconds the galvanometer key is closed. If a deflection is observed on the galvanometer, its key is opened, and the battery key put to earth. The resistances in a and b are then to be readjusted, until, upon successive applications of the battery, no deflection is observed, when, if c = capacity of the standard condenser, the capacity of the cable will be $= \frac{a + c}{b}$

If an electrometer be used, an approximate balance may be obtained without removing the battery, by adjusting the slide until the electrometer indicates zero, after which the battery key should be put to earth, and the condenser and cable recharged, until, by a proper arrangement of the slide, as perfect a balance is obtained as is possible.

A very short time suffices to measure the capacity of a cable, and the method will be found particularly handy on board ship.

If the experiment be made with a Thomson's electrometer, the slide resistance is not absolutely required. Let d^1 = the potential of the battery in divisions of the electrometer scale, d = the deflection when the electrometer is connected to the cable side of the condenser (the other electrode being to earth), then the capacity of the cable $= \left(\frac{d^1}{d} - 1\right) \times$ cap. of condenser.

ST. PIERRE MIQUELON,
May 1st, 1881.

A SIMPLE AND EFFICIENT LIGHTNING CONDUCTOR.

By J. Gott, Member.

A lightning conductor which is thoroughly reliable may be made as follows:—

Solder a smooth metal ball about half-an-inch in diameter, or a wedge-shaped piece of metal with rounded edges, on the end of a piece of G.P. covered copper wire. This wire is to be connected to the cable, instrument, or line it is desired to protect. Stretch tightly as possible a piece of rubber from a toy balloon over the metal ball, and tie it over the G.P.

This ball is then to be immersed in mercury contained in a metal cup, which latter is connected to earth. If the rubber is punctured by a flash of lightning, it contracts, and a tolerably large surface of the metal is instantly in contact with the mercury.

A spark from a small induction coil will exhibit the action of the apparatus.

St. Pierre Miquelon,
May, 1881.

ABSTRACTS.

O. FRÖLICH—TRANSMISSION OF POWER BY DYNAMO MACHINES.

(*Monatsbericht der Königl. Akademie der Wissenschaften zu Berlin*, 18*th Nov.*, 1880.)

The author has undertaken an exhaustive series of experiments on the above question, which, he is of opinion, has not been fully investigated, and gives an account of his results.

I. The Current from Dynamo Machines.

(a) *Equation of Dynamo-electric Equilibrium.*

Ohm's law as applied to a magneto machine, with external resistance in circuit, takes the form

$$I = \frac{n\,M\,v}{R} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

in which I is the current, n the number of convolutions on the armature, v the number of revolutions, R the total resistance in the circuit, M the ratio of the the E.M.F to the number of revolutions, a quantity which the author calls the "effective magnetism." This latter quantity is the sum of the E.M.F. which the permanent magnets and the iron core of the armature exert on one convolution of the armature for one revolution. The same equation holds for dynamo machines, but in this case, since the machine excites its own magnets,

$$M = f(I) \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

while with magneto machines M is nearly constant. Equation (1) when written in the form

$$\frac{I}{n\,M} = \frac{I}{f(I)} = \frac{v}{R}$$

gives the important law that the current is only a function of the ratio of the revolutions to the total resistance. This law holds for all dynamo machines, and for any position of the commutator, and is therefore the fundamental equation. The only quantity which varies with different machines is the product n M. If n is given, and M is known for any one machine, and for a certain position of the commutator, then I can always be calculated from the number of revolutions and the total resistance. If the effective magnetism were simply proportional to the current, the latter would increase to infinity. Hence there is equilibrium only if the magnetism diverges from proportionality with the current, which is in fact always the case. If we call this divergence ϕ (I), then equation (1) becomes, putting $n\,M = c\,I - \phi(I)$,

$$\frac{v}{R} = \frac{1}{c - \frac{\phi(I)}{I}}$$

from which follows that for a certain current the number of revolutions is less, the smaller the divergence of the magnetism from the proportionality. A dynamo machine is the more perfect the nearer the effective magnetism is proportional to the current.

(b) *Proof of the Equation of Equilibrium.*

Numerous experiments were made with one of Siemens and Halske's large D_0 dynamo machines, the number of revolutions and the external resistance being varied. The machine was differently constructed in the several series, as shown in the following table:—

Series.	Electro-Magnets.			Armature.		Total.
	Mean distance from Iron Core.	Convolutions	Resistance.	Convolutions	Resistance.	Resistance.
	mm.		S.U.		S.U.	S.U.
I.	10·5	456	0·290	288	0·145	0·435
II.	21	856	0·580	288	0·145	0·725
III.	14	1,960	4·14	1,296	3·00	7·140

The current was measured by an electro-dynamometer, the constant of which had been determined by electrolysis of copper. The external resistance was formed of long strips of thin sheet iron, which, from the large surface exposed, became only slightly heated by the passage of the current. The brushes on the commutator were placed in every experiment in such a position as to give a maximum current. The author gives tables of the results, which are too extensive to be reproduced, and curves were plotted from them. The experiments showed that essentially the current is a function of the ratio v : R, as equation (2) states. If I were really a function of v : R, then, if for a certain position of the brushes, and a certain value of v and R, a certain current is obtained, this ought not to alter if the number of revolutions and the total resistance are changed in the same ratio. This is, however, not exactly the case, but if v and R are both proportionally increased, the brushes must be advanced slightly in the direction of rotation of the armature to obtain the same current as before. This phenomenon, due to the displacing of the magnetism of the armature by the rotation, is practically of little influence.

Referring to the experiments of Meyer and Auerbach, the author thinks that the deviation of their results from the formula was due to the brushes being kept always in the same position; still, generally, the current appears as a function of the ratio v : R. With the currents required for practical use, the curves approach so nearly to straight lines, that it may be assumed that the current is a linear function of the ratio—number of revolutions : resistance. Accordingly, both in magneto and dynamo machines, the increase of the current is proportional to the increase of the ratio v : R. There is only this important difference between the two kinds, that the magneto machine gives a current

with very slow revolutions, while the dynamo only begins to give a current for a certain value of the ratio $v : R$.

(c) *The Effective Magnetism.*

The effective magnetism at first is proportional to the current, but then always deviates more and more from proportionality, and tends to a maximum. For still more powerful currents the magnetism must gradually diminish; for, if the electro-magnet is magnetised to a maximum, the action of the current on the magnetism of the armature must increase, and therefore the whole effective magnetism must decrease; this, however, only occurs with currents much greater than those likely to occur in practical use. Within practical limits, therefore, we may assume that the effective magnetism reaches a constant maximum. If we put $\frac{v}{R} = a + b\,I$, then

$$I = \frac{1}{b}\left(\frac{v}{R} - a\right) \quad \dots \quad \dots \quad \dots \quad (3)$$

where a is the value of the ratio $\frac{v}{R}$ for which the machine first gives a current, and $\frac{1}{b}$ is the factor of proportionality, and further,

$$M = \frac{1}{n} \times \frac{I}{\frac{v}{R}} = \frac{I}{a + b\,I} \quad \dots \quad \dots \quad \dots \quad (4)$$

in which $\frac{1}{a}$ is the factor of the original proportionality between M and I, and $\frac{1}{b}$ the maximum value of the effective magnetism.

The curves found for the effective magnetism show that the maximum is reached with the machines, as constructed, at a comparatively early stage, and the causes of this early arrival at a maximum are to be sought. It is probable that these causes lie in the magnetic action of the current in the wires of the armature. For this action is opposed to that of the electro-magnets, and in consequence of it the magnetic axis of the armature is rotated, at the same time that the force producing the current is decreased, therefore on the whole the effective magnetism is diminished. To distinguish between the magnetising force of the electro-magnet coils and that of the armature wires, the current from a second dynamo was passed through the magnet coils, the circuit of the armature completed through a high resistance, and the difference of potential of the two poles was measured on the so-called torsion galvanometer. The brushes were placed at the position of maximum effect, which coincided nearly with the plane passing through the two parts of the armature which were out of the magnetic field. The effective magnetism was obtained by dividing the potential by the number of revolutions and by the number of convolutions on the armature. The curves deduced rise more rapidly, and do not show any signs of approaching a maximum value at an early stage. The results experimentally obtained can be readily calculated from the formula (4). The formulæ of interpolation take the following values for the three constructions as given at the commencement for the three machines:—

M is the effective magnetism plus the current in the armature; M_1 the effective magnetism alone.

I. Construction.

$$M = \frac{I}{26500 + 367\,I + 14{\cdot}3\,I^2} \text{ for } 20 < I < 50.$$

$$M = \frac{I}{8070 + 1440\,I};\ M \text{ max.} = 0{\cdot}000694$$

$$M_1 = \frac{I}{14400 + 1040\,I};\ M_1 \text{ max.} = 0{\cdot}000962$$

II. Construction.

$$M = \frac{I}{5180 + 1310\,I};\ M \text{ max.} = 0{\cdot}000763$$

$$M_1 = \frac{I}{9100 + 1010\,I};\ M_1 \text{ max.} = 0{\cdot}00099$$

III. Construction.

$$M = \frac{I}{3200 + 1380\,I};\ M \text{ max.} = 0{\cdot}000725$$

$$M_1 = \frac{I}{5100 + 930\,I};\ M_1 \text{ max.} = 0{\cdot}00108$$

From this it is evident how much the action of the current in the armature diminishes the maximum, viz., by about $\frac{1}{4}$ of its value. These results can also be employed to establish the laws of the effect of the current in the armature on the magnetism. This effect is equal to the difference $M_1 - M$, which is proportional to the number (n) of convolutions on the armature, increases with an increase of current strength, and decreases with increasing magnetism (M_1). Let

$$M_1 - M = n\gamma \frac{I}{M_1}$$

and since

$$M_1 = \frac{I}{a_1 + b_1 I},\quad M_1 - M = n\gamma(a_1 + b_1 I)$$

and

$$M = \frac{I}{a_1 + b_1 I} - n\gamma(a_1 + b_1 I) \quad \dots \quad \dots \quad (5)$$

This formula has been compared with the above-mentioned formulæ of interpolation, and with experiments, and was found to agree closely; the value of γ in these experiments was $\frac{2}{3} \times 10^{-11}$.

Of the two coefficients in formula (5), b_1 is independent of the winding of the coils of the electro-magnets. Since $\frac{1}{b_1}$ means the maximum value of the magnetism, which must be finally reached with any mode of winding, on the other hand a_1, the reciprocal factor of the original proportionality between current and magnetism, is evidently dependent on the winding, as well on the number of convolutions as on their distance from the iron core. From the experiments it was found that $a_1 = \frac{a}{m^q}$, where m is the number of convolutions on the electro-magnets, a and q being constants, for which the values $a = 126{,}000$, $q = 0{\cdot}729$, were found. Hence, finally, the formula for calculating the effective magnetism of a Siemens machine takes the form

$$M = \frac{I}{\frac{a}{m^q} + b_1 I} - n\gamma\left(\frac{a}{m^q} + b_1 I\right) \quad \dots \quad \dots \quad (6)$$

(d) *The Work of Dynamo Machines.*

From Joule's law the work consumed by the machine per second is $A = cI^2R = cIE$, where $c = 0\cdot00181$ according to Kohlrausch, if the work is expressed in horse-power, the E.M.F. in Daniell's, the resistance in Siemens' units, and the current in $\frac{\text{Dan.}}{\text{S.U.}}$ units. If account is taken of the so-called Foucault's currents induced in the iron core, the formula becomes

$$A = cIE + pE^2,$$

and the experiments show that $p = 0\cdot0009$, and that for c should be substituted a constant $c_1 = 0\cdot00163$. The following table contains the results of the experiments. The horse-power required to rotate the machine empty is deducted:—

Revol.	Total Resist.	Current.	E.M.F.	Work.	C.I.E.	C_1.I.E.+p.E².
129	...	14·5	13·6	0·21	0·357	0·34
141	...	20·5	18·2	0 62	0·675	0·64
167	...	29·4	24·9	1·27	1·32	1·25
180	...	32·5	28·7	1·60	1·69	1·59
200	...	37·7	34·8	2·27	2·37	2·25
250	...	46·4	42·1	3·57	3·54	3·34
298	...	53·7	47·7	4·74	4·64	4·39
350	...	59·9	53·3	6·09	5·78	5·46
393	...	65·6	62·3	7·36	7·40	7·01
401	...	66 8	62·4	7·65	7·54	7·14
450	...	72·8	69·2	9·26	9·12	8 64
489	...	74·4	71·8	10·42	9·67	9·17
168	1·35	17·3	22·4	0 63	0·70	0·74
216	...	23·5	31·1	1·33	1·32	1·40
247	...	27·9	36·9	1·89	1·86	1·97
302	...	36·5	49·3	3·21	3 26	3·44
351	...	42·8	58·1	4·37	4·50	4·76
401	...	48·0	66·3	5·53	5·76	6·11
449	...	52·3	73·4	6·82	6·95	7·37
508	...	57·2	82·3	8·39	8·52	9·35

II. The Electrical Transmission of Power.

If it is assumed that the brushes in the two machines, the generator and the motor, are in the same positions, since the same current exists in both, the effective magnetism must be equally strong in both. With this proviso the following formulæ are obtained (E, c and R have their usual meaning, M is the effective magnetism, v the number of revolutions per minute, n the convolutions on the armature, A the work, S the heat produced by the current, N the useful effect, 1 refers to the generator, 2 to the motor):—

$$E_1 = n M v_1\,,\ E_2 = n M v_2$$
$$I = \frac{E_1 - E_2}{R} = M \frac{v_1 - v_2}{R}$$
$$A_1 = c E_1\ I = c I^2 R \frac{v_1}{v_1 - v_2}\,,$$
$$A_2 = c E_2\ I = c I^2 R \frac{v_2}{v_1 - v_2}$$
$$S = c I^2 R,\ A_1 = S + A_2$$
$$N = \frac{A_2}{A_1} = \frac{v_2}{v_1} = \frac{E_2}{E_1}$$

The formulæ do not always agree with the experiments. This is especially the case with the useful effect. From the formulæ this should have a value as high as 90 per cent., for N depends only on the ratio of the two velocities, and the velocity of the motor can increase until it reaches the difference between the velocity of the generator and the velocity for which a current is first produced. In practice, the useful effect reaches 40 to 60 per cent. Further, the work A_2 comes out smaller, and the E_2 greater than according to theory, and the more so the smaller the work A_2 is. The explanation of this decrease is to be found in the so-called Foucault's currents, which are induced in the iron core of the armature. The chief cause of these currents lies in the action of the electro-magnets on the rotating core of the armature. In the generator these induced currents are in the same direction as the currents in the wires of the armature; they weaken the effective magnetism and the E.M.F., and increase the work expended A_1. In the motor the converse is the case. If $c_1,\ c_2$ represent the Foucault currents, u the resistance in their circuit, $M_1,\ M_2$ the effective magnetism, as a first approximation we obtain

$$M_1 = M - \epsilon c_1\,;\ M_2 = M + \epsilon c_2$$
$$c_1 = \frac{M_1 v_1}{u} = \frac{1}{n} \times \frac{E_1}{u}\,,\ c_2 = \frac{M_2 v_2}{u} = \frac{1}{n} \times \frac{E_2}{u}$$

M is here the effective magnetism which would obtain, if the Foucault currents did not exist, and ϵ is a constant. Putting

$$\frac{\epsilon}{u} = \eta_1\ M_1 = M(1 - \eta v_1): M_2 = M(1 + \eta v_2)\ \dots\ \dots \quad (9)$$

$$\left.\begin{array}{l} E_1 = n M_1 v_1 = n M (1 - \eta v_1) v_1 \\ E_2 = n M_2 v_2 = n M (1 + \eta v_2) v_2 \end{array}\right\}\ \dots\ \dots\ \dots\ \dots \quad (10)$$

$$I = \frac{E_1 - E_2}{R} = \frac{n M}{R} \left\{ v_1 - v_2 - \eta (v_1^2 + v_2^2) \right\}\ \dots\ \dots \quad (11)$$

The expressions for the work become

$$A_1 = c n I M_1 v_1 + c c_1 M_1 v_1\,;\ A_2 = \eta I M_2 v_2 - c c_2 M_2 v_2$$

or putting $\frac{c}{n^2 u} = p$

$$\left.\begin{array}{l} A_1 = c I E_1 + p E_1^2;\ A_2 = c I E_2 - p E_2^2 \quad \dots\ \dots\ \dots \\ N = \frac{A_2}{A_1} = \frac{E_2}{E_1} \left\{ 1 - \frac{p}{c I} (E_1 + E_2) \right\} \quad \dots\ \dots\ \dots\ \dots \\ S = c I (E_1 - E_2), F_1 = p E_1^2, F_2 = p E_2^2\ A_1 = A_2 + S + F_1 + F_2 \end{array}\right\} \quad (12)$$

By introducing the values for I_1 W_1 v_1 v_2 these formulæ become

$$\left.\begin{aligned} A_1 &= c\, I^2 R \frac{v_1}{v_1 - v_2} \left\{ 1 + \eta\, v_2 \cdot \frac{v_1 + v_2}{v_1 - v_2} + \frac{p\,R}{c} \cdot \frac{v_1}{v_1 - v_2} \right\} \\ A_2 &= c\, I^2 R \frac{v_2}{v_1 - v_2} \left\{ 1 + \eta\, v_1 \cdot \frac{v_1 + v_2}{v_1 - v_2} - \frac{p\,R}{c} \cdot \frac{v_2}{v_1 - v_2} \right\} \\ N &= \frac{v_2}{v_1} \left\{ 1 + \eta\,(v_1 + v_2) - \frac{p\,R}{c} \cdot \frac{v_1 + v_2}{v_1 - v_2} \right\} \\ S &= c\, I^2 R;\ F_1 = p\, I^2 R^2 \frac{v_1^2}{v_1 - v_2};\ F_2 = p\, I^2 R^2 \frac{v_2^2}{v_1 - v_2} \end{aligned}\right\} \quad (13)$$

From the formulæ (12) the work can be calculated from E_1, E_2, and I_1, whatever may be the position of the brushes; but in using formulæ (13) to deduce the work from the current, resistance and number of revolutions, the brushes must be in the same position on both machines. A full series of experiments was made, the work being measured by a dynamometer, the current by a Siemens' electro-dynamometer, and the difference of potential between the two poles of the machines by a torsion galvanometer: all the measurements were taken simultaneously by six persons, and it was found that the theoretical and the experimental values coincide very nearly. A few examples are appended.

V_1	V_2	E_1 Dan.	E_2 Dan.	I $\frac{\text{Dan.}}{\text{S.U.}}$	$\frac{E_1 - E_2}{R}$ $\frac{\text{Dan.}}{\text{S.U.}}$	A_1 H.P.	Calcd. H.P.	A_2 H.P.	Calcd. H.P.	N %	Calcd. %
603	399	93·1	66·4	30·2	35·7	5·22	5·37	3·12	2·87	60	54
1,013	537	161	105	59·5	60·6	...	17·9	9·44	9·20	...	51
594	452	66	45·8	15·1	15·1	1·99	2·01	0·88	0·94	44	47
727	464	97·8	73·3	18·3	18·4	4·27	3·68	1·81	1·71	43	46
605	343	94·7	65·9	33·7	31·4	6·01	6·01	3·35	3·23	56	54
501	358	57·2	37·0	15·1	15·1	1·70	1·70	0·70	0·79	41	46

C. R. ALDER WRIGHT—ON THE DETERMINATION OF CHEMICAL AFFINITY IN TERMS OF ELECTRO-MOTIVE FORCE.

(Parts III. and IV. Phil. Mag., Vol. II., No. 67, March, pp. 169-196; No. 68, April, pp. 261-283, and No. 69, May, pp. 348-369.)

The author first discusses the most recent valuations of the B.A. unit of resistance, and of the mechanical equivalent of heat, and finally concludes that at present the chances of the B.A. standard ohm being below the true theoretical value, 10^9 C.G.S. units, are about equal to the chances that it is above that value; so that it is assumed by him that the B.A. standard ohm really possesses its nominal value, and consequently that the E.M.F. of Clark's cell is 1·457 volts, as determined by Clark. The same kind of reasoning, however, indicates that Joule's water friction values of the mechanical equivalent of heat

are somewhat too low; the value 42×10^6 represents pretty nearly the most probable value of J, so that the factor for reducing gramme degrees to E.M.F. units (when affinity is calculated from the one form of measurement to the other) is 4,410·0—*e.g.*, the energy measured as heat by 10,000 gramme degrees, is measured by $0{\cdot}441 \times 10^8$ E.M.F. units = 0·441 volts. Clark's cells are found to remain permanent for some few months, especially when put up in hermetically sealed glass vessels rendered vacuous by a Sprengel pump instead of the ordinary paraffin wax-sealed vessels.

The existing state of knowledge is partly discussed as regards the values of the counter E.M.F. set up during electrolysis, of the "subsequent polarisation" (E.M.F. between the electrodes *after* rupture of the current), and of the E.M.F. of gas batteries; and it is shown that the fluctuations in these values introduced by varying the conditions, and various other facts, as also the results of a number of new determinations described at length, are all in accordance with the following general theory. Let it be granted that the first effect of electrolysis is to break up the electrolyte, not into the products finally formed, but into primary "nascent" products, the conversion of which into the ultimate products would be accompanied by heat evolution; then the E.M.F. requisite to break up the electrolyte wholly into nascent products would be a fixed constant amount exceeding that requisite to break it up into the ultimate products. The attractive action of the electrodes, and their chemical action upon the products of electrolysis, however, cause more or less of the "nascent" products to be transformed *ab initio* into other bodies, the amount of such transformation varying with the rate of current flow and other conditions; so that finally the counter E.M.F. set up, *i.e.*, the amount of work actually done in breaking up the electrolyte at the moment, is variable, being less than the limiting maximum by an amount dependent upon the conditions of the experiment. This theory is expressed by the following formula:

$$e = E + \left[\Sigma (1 - n) H - \Sigma (n h) - \Sigma \left(\mathrm{H}\right) \right] \times J,$$

where e is the counter E.M.F. set up, E the E.M.F. representing the work done in breaking up a gramme equivalent of electrolyte into final products, n the fraction of a gramme equivalent of products evolved otherwise than as nascent products (at either pole, severally), H the heat of transformation of nascent into final product, and h the heat evolved by the condensation of the product upon the electrode surface, $\Sigma (\mathrm{H})$ being the algebraic sum of the heat evolutions due to chemical actions between the electrodes and the products, diffusion, &c.

From this formula and the results of a large number of experimental determinations, it results that the E.M.F. of any given electro-motor in which a gas is evolved (like Smee's cell), or a metal deposited (as in Daniell's battery), is a function of the current generated, being less the greater the current. A number of experiments in proof of this are detailed, as are also many others leading to the following general results. The passage of a given quantity of electricity through an electrolyte causes the decomposition of one and the same amount of substance irrespective of the time taken in its passage; in other

words, conduction without electrolysis does not take place, and Faraday's law is true for excessively minute currents as well as for those of considerable magnitude. With very feeble currents, however, and with certain electrolytes, *e.g.*, water, the quantity of products of decomposition actually collected after a given time does not absolutely correspond with the quantity of electricity that has passed, even after obvious sources of suppression have been eliminated, especially occlusion in or condensation on the electrodes, solution in the fluid, or suppression by the chemical action of dissolved gases. The cause of this is the "diffusion discharge" produced (in the case of water) by the diffusion towards the — electrode of water containing dissolved oxygen, and towards the + electrode of fluid containing dissolved hydrogen (and similarly in other cases), thus causing an unavoidable suppression by chemical action. When the amount of suppression due to this cause is determined, and added to the observed amount of decomposition, the total corresponds exactly with the quantity of electricity that has passed.

In the electrolysis of acidulated water, until the sources of loss of hydrogen other than "diffusion discharge" are eliminated (viz., solution in the fluid, condensation on and absorption by the electrode, and action of dissolved oxygen originally present in the fluid), the counter E.M.F. set up when a given steady current traverses a given voltameter is short of its maximum value for that current. Simultaneously a deficiency in the amount of hydrogen collected, as compared with that due to the quantity of electricity passing, is noticed (even after correction for diffusion discharge), whilst on breaking circuit the rate of fall of the "subsequent polarisation" of the electrodes is more rapid than its minimum value for that current. On the other hand, as soon as the counter E.M.F. reaches its maximum, the deficiency in hydrogen disappears (after correction for diffusion discharge), and the rate of fall of the polarisation after breaking circuit reaches its minimum. The more nearly completely the sources of loss are eliminated, the more nearly does the counter E.M.F. set up approach its maximum, the less is the deficiency in the hydrogen collected, and the more nearly does the rate in fall of the subsequent polarisation approach its minimum.

In the case of all the electrolytes examined, the value of the counter E.M.F. set up, e, is found to increase as the current increases, but at a less rapid rate, so that the curves traced out by plotting currents as abscissæ and counter E.M.F. values as ordinates are concave downwards.

So long as the rate of flow per unit area of electrode surface remains the same, the value of e is constant—*i.e.*, if the electrode surface and the current vary in the same way, so that the "density of the current" with reference to the electrode surface is constant, then e remains the same. With a given current, increasing the electrode surface diminishes the value of e.

The values of e depend on the material of which the electrodes are made. Other things being equal, carbon gives higher values than platinum, and platinum than gold.

In the case of acidulated water, rendering the solution more dilute increases the value of e; whilst from former observers' work it results that increasing the temperature decreases e.

By means of the above-cited formula, and the experimental determination of the causes which make e to vary, etc., information can be deduced as to the amount of energy with which gases are condensed upon the surface of solids, and in particular as to the amount of energy requisite to break up an electrolyte into "nascent" products solely. By conjoining this determination with the determination of the methods for finding the E.M.F. corresponding to the energy requisite to break it up into the final products (described in Parts I. and II.), information can be deduced concerning the affinity of the constituents of this electrolyte, and the variations of that affinity according as the constituents are "nascent" or in their ordinary free state. For instance, the energy given out in the transformation of nascent hydrogen and oxygen into the ordinary free gases exceeds the energy that would be developed by the union of these gases to form liquid water, *i.e.*, exceeds the energy equivalent to 34,100 gramme degrees, or to 1·50 volt.

LORD RAYLEIGH AND ARTHUR SCHUSTER—ON THE DETERMINATION OF THE OHM IN ABSOLUTE MEASURE.

(*Royal Society, May 5th,* 1881.)

The unit of resistance in general practical use is the *ohm*, which was constructed, by a Committee of the British Association, to represent 10^9 C.G.S. units. Kohlrausch has found that the ohm is 2 per cent. greater than it was intended to be; Rowland, that it is 1 per cent. too small; and Weber, that it is correct. Lord Rayleigh, having possession of the British Association apparatus, made such alterations in it as were thought necessary, and proceeded to make a measurement according to the original method, which has been employed by no subsequent experimenter. This method consists in causing a coil of insulated wire, forming a closed circuit, to revolve about a vertical axis, and in observing the deflection of a magnet suspended at its centre from the magnetic meridian, the deflection being due to the currents developed in the coil under the influence of the earth's magnetism. The amount of the deflection is independent of the intensity of the earth's magnetic force, and it varies inversely as the resistance of the circuit. The symbols used in the paper to explain the theory are those of the British Association Committee, in whose reports it is fully given. The corrections depending on irregularity of the earth's magnetism at the coil, on imperfect adjustments of the coil and magnet, are exceedingly small. Lord Rayleigh considers carefully the mathematical expressions for self-induction of the coil, and for the field produced at the centre of the coil by unit current. These expressions involve infinite series, and care must be taken that in the working formulæ all important terms are used. He also corrects for torsion of the suspension fibre, and alters very much the nature of the suspension of the magnet. Insulating pieces were inserted in the frame, and tests were made as currents due to rotation when these insulated places were short-circuited; short-circuiting diminished the deflection of the needle by the one-six-hundredth of itself. An important alteration was made in bringing the mirror and the magnet close together in the centre of the coil, there seeming to

be no optical necessity for the old arrangement in which the mirror was above the coil, since only very rapid eclipses of the light are produced by the rotation.

Instead of driving the apparatus by hand and using a governor, the coil being driven for ten to fifteen minutes, and the average speed being taken, a water motor was employed, driven from a cistern, and the speed was taken at any instant by a tuning-fork method. The author gives the theory of water motors, and describes his method of speed measurement at some length. His governor was the friction of an observer's fingers upon the cord of the driving apparatus. Disturbances of the magnet, produced by rotation of the coil when it was not closed, were shown to be due to mechanical vibrations given to the air in the box enclosing the mirror. In taking the changes of the earth's declination during the experiment the simultaneous Kew observations were not used, as at Kew considerable disturbances arise from the passage of steamers on the river, but a separate suspended magnet was employed, placed far enough away from the suspended coil to be sensibly unaffected by it.

It was found that at constant speeds very consistent results were obtained, but the deflection fell off from being proportional to the speed. This result gave great trouble, the coil having to be rewound, and the whole apparatus overhauled, but it was at length found that, although inconsistent with the B.A. reports, the inconsistency was due to an error in the statement of the reports of the correction for self-induction.

Into this question of the correction for self-induction of the coil, Lord Rayleigh enters at great length. He measured it first experimentally, and the theory of his method is simply given as follows. L is the coefficient of self-induction of a coil of resistance P :—

"The arrangement is identical with that adopted to measure the resistance of the coil in the usual way by the bridge. If P be the resistance of the copper coil, Q, R, S, nearly inductionless resistances from resistance-boxes, balance is obtained at the galvanometer when $PS = QR$. This is a resistance balance, and to observe it the influence of induction must be eliminated by making the battery contact a second or two before making the galvanometer contact. Let us now suppose that P is altered to $P + \delta P$. The effect of this change would be annulled by the operation of an electro-motive force in branch P of magnitude $\delta P \,.\, x$, where x denotes the magnitude of the current in this branch before the change. Since electro-motive forces act independently, the effect upon the galvanometer of the change from P to $P + \delta P$ is the same as would be caused by $\delta P \,.\, x$ acting in branch P if there be no E.M.F. in the battery branch at all.

"Returning now to resistance P, let us make the galvanometer contact before making the battery contact. There is no permanent current through the galvanometer (G), but, at the moment of make, self-induction opposes an obstacle to the development of the current in P, which causes a transient current through G, showing itself by a throw of the needle. The integral magnitude of this opposing E.M.F. is simply Lx, and it produces the same effect upon G as if it acted by itself. We have now to compare the effects of a transient and of a permanent E.M.F. upon G. This is merely a question of galvanometry. If T be the time of half a complete vibration of the needle, θ the permanent

deflection due to the steady E.M.F., α the throw due to the transient E.M.F., then the ratio of the electro-motive forces, or of the currents, is

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\theta}.$$

"If, instead of the permanent deflection θ, we observe the first throw (β) of the galvanometer needle, this becomes

$$\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\frac{1}{2}\beta}.$$

"In the present case, the ratio in question is, by what has been shown, above $\delta P.\,x : Lx$ or $\delta P : L$; so that

$$\frac{L}{P}=\frac{\delta P}{P}\frac{T}{\pi}\frac{2\sin\frac{1}{2}\alpha}{\tan\frac{1}{2}\beta}$$

a formula which exhibits the time-constant of the coil P in terms of the period of the galvanometer needle. Further, to deduce the value of L in absolute measure from the formula requires a knowledge of resistances in absolute measure."

[The practical rule to find L, the coefficient of self-induction of a coil, seems to be:—Get a balance in the Wheatstone bridge in the ordinary way, using a very delicate galvanometer, the vibrations of whose needle are not damped. In this way the resistance, P, of the coil is known. The standard coils of the bridge are supposed to have no self-induction. Now press down the galvanometer key before the battery key, thus reversing the usual process. Observe α, the transient throw of the galvanometer needle. Now insert an additional resistance, δP, along with P, and, pressing down the battery key before the galvanometer key in the usual way, measure the first throw, β, of the needle. Measure also the time, T, of half a complete vibration of the needle, and use the measurements in the formula which has been given.]

The value of L originally used is probably too small, for reasons deduced from 1°, the original experiments; 2°, these new experiments; 3°, direct measurement as just described; 4°, Mr. Niven's calculations from the dimensions of the coil. A careful examination of certain errors in the original calculations seems to point to the cause of the discrepancy, and the value for L which is used by Lord Rayleigh is 451,000 metres, instead of 437,000 used by the Committee. There also seems to have been a discrepancy in the determination of the field at its centre, due to unit current in the coil; but at this distance of time it is difficult to discuss the question. Prof. Rowland, in ignorance, apparently, of Maxwell's previous calculation, has shown that if in the original experiments we assume an unknown cause of error proportional to the square of the speed, and eliminate it, we shall arrive at a value of the ohm differing very appreciably from that adopted by the B.A. Committee, or

$$0{\cdot}9926 \times 10^9 \text{ C.G.S. units.}$$

This, then, is the result supported by the original experiments. Rowland's own experiments give the value

$$0{\cdot}9911 \times 10^9 \text{ C.G.S. units.}$$

The paper leaves some vagueness as to the kind of magnet which was used.

The Committee had used a spherical magnet as, when uniformly magnetised, this is known to act like an infinitely small needle at its centre. Lord Rayleigh, near the end of his paper, suggests the use of four little needles placed parallel, as at the corners of a square prism whose length is 2·3 times the side of the square. But Mr. Schuster, in his part of the paper, speaks as if this magnet had actually been used in the investigation. Lord Rayleigh finishes his part of the paper by referring to the new apparatus which has been ordered. "With the new apparatus, and with the precautions pointed out by experience, we hope to arrive at very accurate results, competing on at least equal terms with those obtained by other methods. Most of the determinations hitherto made depend upon the use of a ballistic galvanometer, and the element of time is introduced as the time of swing of the galvanometer needle. There is no reason to doubt that very good results may be thus obtained; but it is, to say the least, satisfactory to have them confirmed by a method in which the element of time enters in a wholly different manner."

Mr. Schuster's part of the paper deals with the actual methods of observation and necessary corrections. Examples of these corrections are given, and an account of some of the difficulties introduced by variations in the resistance of the coil and alterations of the zero of the deflections due to convection air-currents inside the needle case. Four different speeds were generally taken alternately in opposite directions. The absolute resistance of the rotating coil was found by these experiments, and from this was determined the absolute resistance of a certain German silver standard at 8°·5 C. Four different experiments gave for this coil, 4·5888, 4·5886, 4·5878, 4·5885—mean, 4·5883. With minor corrections the mean becomes $4{\cdot}543 \times 10^9$ C.G.S. units, and the coil is found to have 4·591 ohms when that one of the standard B.A. coils which is supposed to have remained most constant is called the ohm. Hence, the final result of these Cambridge experiments is that

$$\text{One ohm} = 0{\cdot}985 \times 10^9 \text{ C.G.S. units.}$$

J. J. THOMSON—ON THE ELECTRIC AND MAGNETIC EFFECTS PRODUCED BY THE MOTION OF ELECTRIFIED BODIES.

(*Philosophical Magazine, Vol. II., April*, 1881, *pp*. 229-249.)

The author refers to the experiments which have been made by Mr. Crookes and Dr. Goldstein on the behaviour of particles of matter largely charged with electricity and moving with great velocities. They have an action on one another, and they are influenced by the presence of a magnet. He proposes to consider mathematically the problem:—According to some theory of electric action, what is the force acting between two moving electrified bodies, what is the magnetic force produced by a moving electrified body, and in what way is the body affected by a magnet? He assumes Maxwell's theory to be true—that variations in the electric displacement in a dielectric produce effects analogous to those produced by ordinary currents flowing through conductors.

The charged bodies are supposed to be spherical for simplicity. Th propositions proved are as follow :—

A sphere of radius, a, moving through unlimited space, filled with a medium whose specific inductive capacity is k, and magnetic permeability μ, experiences a resistance to motion, because at any instant at any point the electrical displacement in the dielectric is changing, on account of the position of the sphere changing. A changing displacement is what constitutes a current, and this current is an evidence of energy which has been created at the point. Work must have been done, then, upon the sphere, if it started from rest, greater than the mere kinetic energy which it mechanically acquires, and the effect is the same as if the mass of the sphere had been increased by the amount

$$\frac{4}{15} \frac{\mu e^2}{a}$$

There is no need to assume waste of energy in heat, because, although currents are produced, the resistances are infinitely small.

In the case of the earth, assuming an electrical charge such as Dr. Macfarlane's experiments allow us to assume, the increase in mass required by the formula would be 650 tons. For spheres of different mass the greatest increase in mass varies as the cube of the radius; hence the ratio of this increase to the whole mass of the sphere is constant for all spheres of the same material; for spheres of different materials the ratio varies inversely as the density of the material. He also finds that, if v is the velocity of the sphere, it produces the same magnetic effect in the medium as a unit length of the current

$$\mu e v$$

situated at the centre of the sphere, the direction of the current being the same as the direction of motion of the sphere. That is, the resultant magnetic force at any place is $\mu e v \sin. \epsilon \div r^2$ if ϵ is the angle between the direction of motion of the sphere and the direction of the line of length r joining the point in question with the centre of the sphere, and the force acts at right angles to these two directions.

An explanation of the phosphorescence observed in vacuum tubes, different from the cause assigned by Mr. Crookes, is that when an electrified particle strikes the glass it changes the direction of its motion, and therefore there is a rapidly varying electro-motive force in the glass. This is exactly what, according to Maxwell's theory of light, glass is subjected to when a beam of light falls upon it, and this is the ordinary method of exciting phosphorescence. An objection to this theory is that the whole tube ought to phosphoresce, but the author shows that Stokes' law of phosphorescence and the investigations of Spottiswoode and Moulton dispose of this objection. He also explains an experiment of Goldstein, which showed that, if the particles are stopped by a very thin layer of collodion covering the glass, the glass does not phosphoresce; the calculated varying electro-motive force is as the square of the distance from the glass of the screen which stops the particles.

The author next investigates the deflection produced by a magnet on the path of an electrified air particle, and then the force between two such particles,

and he deduces a law which differs materially from Weber's law. According to Weber's law, the force between two particles does not depend on the actual velocities of the particles, but only on their velocity relative to each other; whereas, according to the theory,* the forces depend on the actual velocities of the particles as well as on their relative velocities: thus there is a force between two charged particles moving with equal velocities in the same direction, in which case, of course, the relative velocity is nothing. Of course the actual velocity of a particle, as taken in the paper, is its velocity relatively to the medium in which it is moving. If Clausius' notion of a current is accepted, it follows from Maxwell's theory that the electro-dynamic phenomena produced by a current depend on the magnetic permeability, but do not depend on the specific inductive capacity of the medium. This agrees with Faraday's experiments. He found, using media whose magnetic permeabilites were all nearly unity, that although their specific inductive capacities were very different, they seemed to produce no change in the magnetic effects produced by the current.

BOLTZMANN—THE THEORY OF GAS FRICTION.†

(Kaiserliche Akademie der Wissenschaften in Wien, Math. Naturw. Classe, Jan. 15, 1880, pp. 11-13.)

The author towards the end of his memoir shows that Mr. Hall's experiments‡ enable us to calculate the absolute velocity of electricity in a current. If the gold leaf of length l, breadth b, is in a uniform magnetic field of intensity m, the electro-magnetic force acting on it at right angles to the lines of force is

$$k = \frac{m\,l\,J_e}{v}$$

if J_e is the current in electrostatic measure, and v is the ratio of electro-magnetic to electrostatic units. If in time t a quantity of electricity e passes through the cross-section of the gold leaf with a velocity c, then $J_e = \frac{e}{t} = \frac{e\,c}{l}$ and hence $k = \frac{m\,e\,c}{v}$.

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* The actual result found by the author is this:—If charges on the spheres are e and e', μ the magnetic permeability of the medium, R the distance between the particles, q and q_1 the velocities of the spheres, and ϵ the angle between their directions of motion, if $\dot{q}_1$ is the *acceleration* of the second sphere, then the forces acting on the first sphere are an attraction

$$\frac{\mu e e'}{3 R^2} q q_1 \cos. \epsilon,$$

along the line of centres; a force,

$$\frac{\mu e e'}{3 R} \dot{q}_1$$

in the direction opposite to acceleration of second sphere; a force,

$$\frac{\mu e e'}{3} q_1 \frac{d}{d t} \left(\frac{1}{R}\right)$$

in direction opposite to direction of motion of second sphere. It will be seen from this that the forces acting on the particles are not equal and opposite.

† This and the following four abstracts relate to the "Hall" effect.

‡ See abstract, *Journ. Soc. Tel. Eng.*, Vol. IX., 1880, p. 66.

If now there is a difference of potential p at two places whose distance apart is b, then the force acting on the quantity of electricity e is $\frac{p\,e}{b}$. Hence in Hall's experiment $k = \frac{p\,e}{b}$ or $\frac{m\,e\,c}{v} = \frac{p\,e}{b}$ or $c = \frac{p\,v}{m\,b}$.

From this formula the absolute velocity c of the electricity in the current may be determined. It is the velocity with which a wire of length b must be moved, perpendicular to itself, in the field to generate in the galvanometer circuit the actual current measured.

E. H. HALL—ON BOLTZMANN'S METHOD FOR DETERMINING THE VELOCITY OF AN ELECTRIC CURRENT.

(*Silliman's American Journal, Vol. XIX.*, 1880, *pp.* 52-54.)

The author refers to the paper of which an abstract has just been given, and thinks that there is a fundamental error in Boltzmann's reasoning. To make this error clear, he shows that on Boltzmann's assumption that the electro-magnetic force acting at right angles to the lines of force in the field ought to be equal to the force, due to electrostatic action, acting on the electricity in the gold leaf, the amount of this force in the *direction* of the primary current in the gold leaf ought to be exceedingly great—600,000 dynes per centimetre of gold leaf strip in a special case which he considers. He also considers that his more recent discovery of the change of sign of the observed electro-motive force in iron gives another objection to the theory.

ROWLAND—PRELIMINARY NOTES ON MR. HALL'S RECENT DISCOVERY.

(*American Journal of Mathematics, Vol. II., pp.* 354-356.)

The recent discovery by Mr. Hall of a new action of magnetism on electric currents opens a wide field for the mathematician, seeing that we must now regard most of the equations which we have hitherto used in electro-magnetism as only approximate, and as applying only to some ideal substance which may or may not exist in nature, but which certainly does not include the ordinary metals. But as the effect is very small, probably it will always be treated as a correction to the ordinary equations. The facts of the case seem to be as follows, as nearly as they have yet been determined:—Whenever a substance transmitting an electric current is placed in a magnetic field, besides the ordinary electro-motive force in the medium, we now have another acting at right angles to the current and to the magnetic lines of force. Whether there may not be also an electro-motive force in the direction of the current has not yet been determined with accuracy, but it has been proved within the limits of accuracy of the experiment that no E.M.F. exists in the direction of the lines of magnetic force. This E.M.F. in a given medium is proportional to the strength of current and the magnetic intensity, and it is reversed when either

the primary current or the magnetism is reversed. It has also been lately found that the direction is different in iron from what it is in gold or silver. The author shows that if we draw a line AB to represent the original current at A, and that AC at right angles to AB represents the new effect, then AD, the resultant of AB and AC, is the total resultant effect, which is really the same as if the current were rotated at the point A by an amount proportional to the magnetic force. From this he thinks he sees an explanation of the rotation of the plane of polarisation of light in the magnetic field; and, if this is true, it constitutes a much needed and a very important link in Maxwell's electro-magnetic theory of light. The explanation is not yet complete, for two reasons: 1st. The quantitative law of Mr. Hall leads to the rotation of the polarisation plane being inversely proportional to the wave length, whereas it is really as the square or cube; 2nd. Hall's experiment has not yet been extended to displacement currents in dielectrics. He will have to show that the lines of electrostatic action are rotated round the lines of force as well as the electric currents.

E. H. HALL—ON THE NEW ACTION OF MAGNETISM ON A PERMANENT ELECTRIC CURRENT.

(*Silliman's American Journal, Vol. XX.*, 1880, pp. 161-186.)

In his former paper* the author had confined his attention to experiments with gold leaf. This paper is mainly devoted to experiments with other conducting substances. He first mentions experiments, which suggested themselves, to detect whether the cross electro-motive force set up by the magnetic field increases the resistance of his conductor, but which have not yet been tried, or else have led to inconclusive results. Professor Rowland suggested an experiment to discover whether the magnetic field affected the static induction in glass. A 4 cm. square block of glass had a depression made in each of four of its faces, all in one plane. Into these depressions plugs of brass, A, B, C, D, were cemented. A and C were maintained at a very considerable difference of potential; B and D were connected with a quadrant electrometer. No change in the deflection of the needle of the electrometer was observed on reversing the nature of the magnetic field in which the glass was placed, and hence, if there is any action of this kind, a change of potential of B and D, it must be less than one forty-thousandth of the difference of potential of A and C.

The author now goes on with an account of the development of his former investigation. If E is the difference of potential per centimetre in the longitudinal direction in his strip, and E_1 in the transverse direction, M being the strength of the magnetic field, he thought, from his former experiments, that $E\,M \div E^1$ would be constant, not only for different strips of the same metal, but for all conductors. This is not the case, and he prefers to use the term $M\,V \div E^1$ where V is the primary current divided by the area of cross-section of the strip. But he is dissatisfied with the employment of this function in

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* See abstract, *Journ. Soc. Tel. Eng.*, Vol. IX., 1880, p. 66.

comparing different metals. V is of course the quantity of electricity passing through unit area of the cross-section per second. He finds perfect uniformity as to the direction of the derived current in different strips of the same metal, a fact of fundamental importance. In accordance with Rowland's prediction, the direction in an iron strip is opposite to what it is in gold and silver. Nickel, which ought to act like iron, behaves like gold. The author describes fully his experiments with each metal, his difficulties in making measurements of thickness of the metal, etc., but his results may be briefly given. In seventeen experiments with four gold specimens, he gets values of $\frac{M\ V}{Ei}$ ranging from 164×10^{10} to 123×10^{10}. He made fewer experiments with the other metals, but the average proportional values for $\frac{M\ V}{E}$ are as follows:—

Iron	— 78
Silver	8·6
Gold	6·8
Platinum	2·4
Tin	0·2 (?)

There seems to be no connection in proportional magnitude between the effect discovered by Mr. Hall and any other physical phenomenon. The fact that nickel and platinum, magnetic bodies like iron, behave in an opposite way, is a remarkable anomaly, of importance in connection with the rotation of the plane of polarisation of light on Maxwell's theory; and it was suggested that there might be a similar difference from iron in the behaviour of nickel in its reflection of polarised light, but this he found not to be the case. He suggests a mechanical illustration of his notion of the action. He also gives at the end of his paper the previous history of the subject—a history quite unknown to him when he began his experiments. Feilitzsch got no result when he tried if a third coil with a current flowing in it produced any permanent effect on the current in one of two other coils which already had currents flowing in them balanced in a differential galvanometer. Mach tried whether the current-stream lines in a disc of silver leaf were altered in shape or position by a magnetic field. In 1874, Gore tried to increase the current through one limb of a partly bifurcated wire by means of a magnet, and failed to get any result. Wiedemann, in his *Galvanismus*, describes the very apparatus which Mr. Hall uses, as a means of showing that no such action occurs. The same plan was devised by Rowland in 1876.

J. HOPKINSON—NOTE ON MR. E. H. HALL'S EXPERIMENTS.

(*Philosophical Magazine, Vol. X.*, 1880, *pp.* 430, 431.)

The author calls attention to the fact that Prof. Clerk Maxwell, in his treatise on electricity (Vol. I., p. 349), has introduced the term T, which he calls the *rotatory coefficient* of a substance, in his equations expressing the currents which flow in any conductor under the action of an electromotive force. Maxwell had reason to believe that it did not exist in any

known substance, but we now see that it does exist; and the results of Hall's experiments are perfectly expressed by saying that there is a direct action of a magnetic field upon the conductor itself changing its coefficients of resistance, instead of saying that the effect is produced on a steady current itself, as distinguished from the conductor.

CARL FROMME—E.M.F. OF VARIOUS CELLS WITH ZINC AS NEGATIVE PLATE.

(*Annalen der Physik und Chemie*, *B. XII.*, *H.* 3, *No.* 3, 1881, *pp.* 399-425.)

1. The author was led to undertake an extended course of experiments on the E.M.F. of cells consisting of zinc, $H_2 SO_4$ and some other metal, from a consideration of Exner's conclusions from his experiments.

2. The E.M.F. of the various combinations were compared with that of a standard Daniell cell, containing 5 per cent. solution of $Zn\ SO_4$ and 11·5 per cent. solution of $Cu\ SO_4$, the measurements being taken on a Kirchhoff's quadrant electrometer with telescope and scale. The standard cell was made up by placing a copper and a zinc cylinder in two glass tubes, drawn out below to a point, the one filled with $Cu\ SO_4$, the other with $Zn\ SO_4$. These tubes are passed air-tight through the cover of a beaker glass, and dipped into a solution of $Zn\ SO_4$. Copper leading wires were soldered on to the two cylinders. Connection with the electrometer was made by a mercury commutator. The metals Pt, Au, Ag, Cu, and gas coke were severally used in combination with Zn and $H_2\ SO_4$.

3. The experimental cell was used in two forms: either each metal was in a separate vessel, the two being connected by a syphon, or both metals were in the same glass. Gold, silver, and copper were used only in the form of wires, platinum as wire and in thin sheets, carbon in small rods. The zinc was always amalgamated, and in form of a cylinder. With the first form of cell, the following values were obtained as mean of all the observations, the E.M.F. of a Daniell being taken as unit:—Zn-Pt, 1·507; Zn-Au, 1·485; Zn-C, 1·374; Zn-Ag, 1·314; Zn-Cu, 0·977. This value for the combination Zn-Pt can only be obtained by most carefully cleaning the platinum, and freeing it from all hydrogen, which may be most effectually done by heating it to redness in the flame of a spirit-lamp. The author considers that the low value (1·15) obtained by Exner for a Smee's element was due to unclean platinum: Beetz found 1·52. Similarly, Beetz's value for Cu (0·98) agrees with the above, while Exner found 0·827. The values for Ag and carbon also agree closely with those found by Beetz. These experiments show that while each metal has a constant E.M.F., which is independent of the quantity of oxygen dissolved in the liquid, the E.M.F. of the various metals varies from 0·977 to to 1·507.

4. In the second arrangement, where both metals were in the same vessel, the same values were found for carbon, Ag and Cu, but lower ones for Au and Pt. This is due to the presence of hydrogen set free by the gradual solution of the zinc. The hydrogen has the greatest effect on platinum, as has long been

known, less on gold, and apparently none on carbon, silver, and copper. By direct experiment it was found that, if hydrogen were electrolytically set free in a Zn-Pt cell by means of a second pair of electrodes, the E.M.F. sank to 0·708 D. This minimum value depends on the area of the plate used: the smaller the area the more surely is the minimum reached. It follows that, if it is found that a cell, Zn-Pt, has an E.M.F. of 0·708 D, the platinum has taken up the maximum quantity of hydrogen.

5. Further experiments were made by short-circuiting the cell, and measuring the E.M.F. after the current was broken. In every case the zinc had stood for 12 hours in the dilute acid. The following table gives the values obtained:—

	Before Short-circuiting.	10''	20''	30''	40''	60''	80''	100''
		After Interrupting Current.						
Pt wire ...	Sc. 35·5 (0·71 D)	...	33·2	...	33·6	33·9	...	...
Pt wire ...	Sc. 35·0 (0·70 D)	...	33·3	...	34·0	...	...	...
Pt wire ...	Sc. 34·5 (0·69 D)	...	33·0	33·2	33·3	...	33·4	...
Au wire ...	Sc. 68·8 (1·376 D)	...	24·8	25·6	26·2	28·0	29·5	30·5
Ag wire ...	Sc. 58·4 (1·168 D)	...	38·7	42·0	43·8	46·4	...	...
Cu wire ...	Sc. 49·0 (0·98 D)	19·0	33·5	...	43·5	...	...	...
Carbon ...	Sc. 114·0 (1·380 D)	...	29·5	34·0	37·0	46·5	...	...

It is to be remarked that in the case of platinum the first throw of the needle was more (36-37 Sc.), and that after a few swings it settled at a constant value; with Au, Ag, and carbon, the needle came back very little, and then was further and further deflected; while with Cu there was a continual increase from the beginning. It appears from this that in the case of platinum either the polarisation is small, or it disappears very quickly; while with carbon, Au, Ag, and Cu, the E.M.F. in the closed cell is small in comparison with that obtainable some little time after interrupting the current.

6. The author, not being satisfied with this method, sought for some means of measuring the E.M.F. whilst the cell was short-circuited. In a wide glass vessel, A, was a zinc cylinder, and outside of this the metal to be experimented on. A glass syphon drawn out to a point connected a second vessel, B, with A in the straight line joining the zinc and the metal, and on the side nearest to the latter; in B was also a zinc cylinder; the vessels and syphon were filled with dilute H_2SO_4. The two metals in A, *e.g.*, Zn and Pt, can be connected in the circuit through a rheostat. The Pt is in connection with the earth and one

pair of quadrants of the electrometer, the Zn in B with the other pair. By this arrangement the difference of potential between the Pt or other metal in A and the Zn in B could be measured while a current circulated in A. To prove the correctness of this arrangement, the Zn in A was put to earth, while a copper wire was in B; the difference of potential between the Zn and Cu measured 0·98 D. If now the Zn and Pt in A were connected up, the deflection on the electrometer should remain the same in spite of the current in A if the arrangement were right and the Zn remained unpolarised. It was found, however, that on closing the circuit the deflection decreased: this was due only to the polarisation of the zinc, and not to any faultiness in the method. The following was the course of experiment:—Both vessels were filled fresh, and the two zinc cylinders and the other metal put in. The E.M.F. between the latter and the zinc in B was then measured, first, when no current passed in A; the two plates in A were then joined successively through resistances from 0 to 9,000 S.U., and the deflection read for each resistance; finally, the current was interrupted and further readings taken. The results are tabulated below. The second series were taken on another day, when the sensitiveness of the electrometer was different. Pt_1 and Pt_2 were naked wires; Pt_3, Pt_4, and Pt_5, wires fused into glass tubes; Pt_6 was a platinum plate.

Zn and	Area Immersed	$R = \infty$.	$R = 0$.	$R = 500$.	$R = 2,000$.	$R = 4,000$.	$R = \infty$ 20″ after interrupting.	$R = \infty$ 40″ after interrupting.
	Sq. mm.							
Pt_1	90	1·515	0·175	...	0·673	...	0 854	0·868
Pt_2	110	1·434	0·183	...	0·664	...	0·844	0·858
Au	110	1·423	0·064	...	0·273	0·395	0·807	0·971
Cu_1	100	0 990	0·071	...	0·344	0·402	0.967	0·981
C	...	1·362	0·169	0·586	...	...	0·638	0·660
Pt_3	9	1·526	0·100	...	0·584	0·654	0·775	0·789
Pt_4	37	1·475	0·153	...	0·650	0·698	0·822	0·840
Pt_5	1	1·397	0·025	...	0·253	0·394	0·801	0·851
Pt_6	4,500	1·483	0·395	...	0·772	0·817	0·830	0·833
Cu_2	40	0·982	0·035	...	0·268	0·378	0·951	0·961
Ag	110	1·218	0·163	...	0·514	0·682	0·989	1·047

7. The E.M.F., before making circuit for Au, Ag, Cu, and carbon, agree well with the means given in section 3. The differences in the case of the platinum were due to the polarisation with hydrogen. For any resistance the E.M.F. of the Zn-Pt element is less, the smaller the surface of the platinum. If the current is broken, E.M.F. are momentarily obtained which are almost equal for large and small surfaces. The double alternative mentioned in section 5 is therefore settled, that the E.M.F. of a Zn-Pt element is smaller when the circuit is closed than when open, but on breaking circuit it increases rapidly. The difference between 1·507, the mean value for a Zn-Pt element when the

circuit is open, and the values when it is closed through a resistance = 0 gives for Pt_6 1·112, for Pt_3 1·407, for Pt_5 1·482, which are the E.M.F. due to the hydrogen polarisation, but are not the maximum values of the same. The author concludes that the maximum value is higher than has hitherto been assumed, and it is necessary to measure the polarisation whilst the current still passes, owing to the rapidity with which it disappears, as shown in the following table:—

	R = 0.	20″ after interrupting.	40″ after interrupting.
Cu	0·93	0·02	0·01
Ag	1·08	0·23	0·17
Carbon	1·19	0·72	0·70
Pt	1·33	0·69	0·67
Au	1·36	0·61	0·45

From a consideration of the curves plotted from the values he has found, the author is of opinion that the E.M.F. increases in greater ratio to increasing resistances, the greater the surface of the metal; and that for the same external resistance the E.M.F. approach more and more the same value with increasing resistance; or, in other words, the E.M.F. decreases with the smallness of the surface, therefore the polarisation increases.

8. In section 3, the author has defended the view that if Pt is brought into H_2SO_4 containing hydrogen, the hydrogen is condensed on the platinum, and the potential between the Zn and Pt decreases. The following experiment does not seem at first sight to be in accord with this view:—Two vessels, A and B, were connected by a syphon; both contained H_2SO_4. A was saturated with hydrogen and held a cylinder of zinc, B was free from hydrogen and had in it a platinum wire. In this arrangement the potential measured 1·507 D. The platinum was then brought into A with the zinc, and the potential measured only 0·70 D, but on restoring the Pt to B, a value rather above 1·5 was again obtained. Supposing that the E.M.F. remains constant, this may be explained. The platinum condenses the hydrogen in the vessel A, but only to a small extent. Moreover, the condensation is only a surface one, and does not extend into the substance of the metal. On removal to B, oxygen is condensed, and, combining with the hydrogen, does away with the polarising action of the latter.

9. The differences obtained in the experiments described in section 3 are too great to be accounted for by errors of observation, but may be due to the different degrees of concentration of the sulphuric acid. The author has instituted a series of researches into the question of the influence of concentration of the acid on the E.M.F. A cylinder of zinc stood in one vessel of dilute H_2SO_4, connected by a syphon with another, which could be replaced at pleasure without disturbing the apparatus, and which contained the solution to be experimented on. A drop of concentrated H_2SO_4 was taken up on a glass rod, and then the rod stirred in a vessel containing $\frac{1}{20}$ litre of water.

With this extremely dilute solution the E.M.F. changed as shown :—

For	Zn-Pt	from	1·347	to	1·508
„	Zn-Au	„	1·300	„	1·452
„	Zn-Ag	„	1·334	„	1·262
„	Zn-Cu	„	1·095	„	1·016

With a trace of acid the E.M.F. of Pt and Au reaches a maximum value, at which it remains on further addition of H_2SO_4; it then falls to a minimum value; increases rapidly again with further concentration; and finally falls off again a little. For Cu and Ag the case is different: with Ag the E.M.F. continually decreases as the concentration increases; with Cu the minimum value is reached with a degree of concentration of 60-70 per cent., and rises slightly again afterwards.

10. The author recurs to Exner's experiments. The latter has stated that any combination gives an E.M.F. of 0·732 D if the H_2SO_4 is free from air. Exner, however, not only got rid of all air from his cells, but he charged them with hydrogen, which falsified the results. The author points out that Exner's results do not agree with his own and those of Beetz. He is of opinion that the differences observed in the case of Pt are due to the differing densities of the hydrogen condensed on the metal.

W. BEETZ—ON THE CONCEPTION "GALVANIC POLARISATION."

(Annalen der Physik und Chemie, B. XII., H. 3, No. 3, 1881, pp. 474-477.)

The following remark occurs in the treatise by Ayrton and Perry "on Exner's works in contact electricity" (*Phil. Mag.*, 4, 11, p. 53, 1881):—"The word polarisation is used in England in the most indefinite way to express an alteration of the current, whether this arises from an alteration of E.M.F. or of the resistance, but in Germany many physicists unite a distinct conception with it." However strange this remark may sound, there is real ground for it. For instance, the definition of polarisation in Fleeming Jenkin's "Electricity and Magnetism" is given in the following terms:—"The expression polarisation is sometimes very vaguely employed, but apparently here it means that the plates become coated with the products of the decomposition of the electrolyte, and that this coating produces a diminution of current. This diminution does not, however, *appear* to be due to anything analogous to resistance. The effect in question is due to something in the nature of a reciprocating force by which energy is stored up, *i.e.*, when the original current ceases, a current in the opposite direction is set up at these surfaces of passage from liquid to solid by a kind of rebound. It *appears*, therefore, that the current has been diminished by the creation of an opposing E.M.F. due to the arrangement of the elements into which the electrolyte has been decomposed. The term resistance is, however, *continually* applied to this cause of the diminution of a current, even by those who are convinced that the diminution is not due to a true resistance." This uncertain mode of definition, with its "appears" and its incorrect conclusion, is the more strange, since Jenkin immediately goes on to give the well-known

experiment that the existence of a secondary after breaking the primary current undoubtedly points to electro-motive force. Moreover, that this uncertainty is not general even in England, is shown by the law with which Clerk Maxwell introduces his chapter on polarisation:—"If an electric current has passed through a liquid between metal electrodes, the accumulation of the ions causes the phenomenon called polarisation, which consists in an E.M.F. which acts in the opposite direction to the current, and produces an *apparent* increase of the resistance." Jenkin means the same thing, but Maxwell has expressed himself so clearly as to leave no room for doubt. In fact, this view of polarisation is not dependent on any theory; and it is only the causes of it which admit of discussion by the partisans of the chemical or contact theories. It is much to be wished that in England, as elsewhere, the term "polarisation" should no longer be used "in the vaguest way" for every sort of diminution of current, but only for the secondary E.M.F. at the electrodes.

That new resistances are formed during electrolysis is a distinct fact, which should not be confounded with the production of a secondary E.M.F. Ayrton and Perry, also, do not hesitate to regard polarisation as an alteration of the electro-motive condition of the electrodes. After referring to the author's experiments as described in the *Annalen* (10, p. 368, 1880), they go on to say: "The nature of the negative metal must always have a great influence on the original E.M.F.; yet we are surprised that Prof. Beetz has found so great differences in the columns headed 'closed.' Our impression has always been that if the negative metal is sufficiently covered with hydrogen, the contact between the thin conducting layer of gas and the metal was equivalent to that of a pair of metals, and if this is the case, then H | Pt + Pt | Zn is the same as H | Cu + Cu | Zn; and almost every negative metal would eventually act like a conducting plate of hydrogen." The author agrees with this view; but even this sufficient covering with hydrogen does not result in such a manner as though one had to do with a simple plate of hydrogen. Buff has said: "Through the layer of hydrogen on the negative platinum plate, as well as through the layer of oxygen on the positive plate, the same result is reached, as though not two strips of platinum, but a strip of fixed hydrogen and a strip of fixed oxygen had been dipped in the acid. . . . . . This limit will always be approached more closely, the more thoroughly the immersed plates are able to condense the gases, and the more thoroughly the immediate contact of the metallic and liquid conductors is avoided. If the immersed plates could be entirely insulated from the liquid, the chemical nature of the metals would be indifferent." While the author, in his work on the E.M.F. of gases, followed this view, he adds: "But, because even with the maximum of polarisation the metals are not equivalent, a complete covering of the plates with the gases must not be assumed, and the values of the polarisation must not be considered as the true E.M.F. of the gases concerned."

ERASMUS KITTLER—DIFFERENCES OF POTENTIAL BETWEEN FLUIDS IN CONTACT.

(*Annalen der Physik und Chemie, B. XII., H. 4, No. 4*, 1881, *pp.* 572-590.)

The experiments which have been made by Fechner, Wild, Du Bois-Reymond, and others, have usually been carried out in the following way:—Of four vessels, A, B, C, D, which can be connected by syphons, the two outer ones, A and D, are filled with the same liquid F, while B and C are filled with different liquids, F_1, F_2. In A and D dip plates of similar metal in circuit through a galvanometer. If there is a current, it follows that the liquids F, F_1, F_2 act electrically on one another, and at the same time that they are not arranged in an electro-motive series; but if the electricity remains in equilibrium, either there is no difference of potential, or they follow Volta's law of tension.

The investigation may be made in another way. Suppose three liquids, F_1, F_2, F_3, which, in combination two at a time with a metal M, give the E.M.F., a, b, c, viz.—

$$M \mid F_1 + F_1 \mid F_2 + F_2 \mid M = a$$
$$M \mid F_2 + F_2 \mid F_3 + F_3 \mid M = b$$
$$M \mid F_1 + F_1 \mid F_2 + F_2 \mid M = c$$

If now the experiment shows that $a - b = c$, then F_1, F_2, F_3 form an electro-motive series. In this case

$$a - b = M \mid F_1 + F_1 \mid F_3 + F_3 \mid F_2 + F_2 \mid M$$
$$= c = M \mid F_1 + F_1 \mid F_2 + F_2 \mid M$$

therefore $$F_1 \mid F_3 + F_3 \mid F_2 = F_1 \mid F_2$$

But if $a - b$ does not $= c$, the three liquids do not form an electro-motive series.

The author carried out this method of investigation with solutions of various chlorides, which were brought into contact among themselves, or with distilled water, copper sulphate, or dilute acid. The E.M.F. were measured by comparison with a Daniell's cell by means of a quadrant electrometer by Edelmann. The needle was charged by a small Zamboni's pile, which remained constant for months. The Daniell's cell, of the following construction:—

$$\text{Amalg. Zn} \left| \begin{array}{l} H_2SO_4 \\ \Delta = 1{\cdot}074 \end{array} \right. + H_2SO_4 \left| \begin{array}{l} Cu\,SO_4 \\ \text{conc.} \end{array} \right. + Cu\,SO_4 \,\Big|\, Cu$$

gave as a mean a deflection of 90 to 91 mm. on the scale used for reading the deflections with a telescope. Every care was used to obtain the chemicals as pure as possible, and in the making of the solutions, the densities of which were exactly determined, and the percentage. For experiments in the same series, distilled water from the same bottle was always used, as it was found that, when different samples were employed, incorrect results were obtained. The chief difficulty was in electrodes. In the first experiments, rods of carbon were used; afterwards plates of silver and copper wires; occasionally zinc rods, which were always freshly amalgamated. The liquids were contained in glasses 6 cm. high and 4·5 cm. wide, which could be put in communication by syphons closed with parchment paper. In each experiment the whole apparatus

was renewed. Before and after each series of observations, the deflection with a Daniell's cell was determined, the copper pole being put to earth, and the zinc connected to the electrometer.

The numerous experiments made by the author lead to the following laws :—

1. The solutions in water of the chlorides $NH_4\,Cl$, $K\,Cl$, $Na\,Cl$, $Ni\,Cl_2$ follow very nearly Volta's electro-motive series, whether dilute solutions of different metals are in contact, or solutions of different strength of the same chloride.

2. The same holds good when the chlorides are in contact with distilled water or solution of copper sulphate.

3. To the difference of potential expressed by

$$M \mid L_1 + L_1 \mid L_2 + L_2 \mid M = + e$$

corresponds a current in the direction from L_1 to L_2. (*a*) If L_1 and L_2 are solutions of the same chloride, but L_2 of less specific gravity, the current goes from the heavier to the lighter solution. (*b*) If L_2 is replaced by distilled water or $Cu\,SO_4$, the current is from the chloride to the water or $Cu\,SO_4$. (*c*) The direction of the current is the same, whether the electrodes are of silver, copper, or zinc.

4. The above chlorides in contact with acids do not follow Volta's electro-motive series.

5. In the combination

$$\left.\begin{matrix}Ag\\Cu\end{matrix}\right\} L + L \mid H_2\,SO_4 + H_2\,SO_4 \left\{\begin{matrix}Ag\\Cu\end{matrix}\right. = e$$

the current goes from the chloride to the acid; and e is a maximum when L is concentrated. But with the zinc electrodes, the current is from acid to chloride, and while e is small for a concentrated solution, it increases on dilution.

6. The combination

$$\left.\begin{matrix}Ag\\Cu\end{matrix}\right\} L + L \mid H\,Cl + H\,Cl \left\{\begin{matrix}Ag\\Cu\end{matrix}\right. = e$$

gives a current from chloride to acid when the chloride is concentrated, and in the opposite direction with a dilute solution.

7. The law of electro-motive series holds for various solutions of zinc sulphate in contact with water.

8. In the combination

$$Zn \mid L_2 + L_2 \mid L_1 + L_1 \mid Zn = + e$$

the current is from the dilute to the concentrated solution; in the combination

$$Zn \mid L + L \mid W + W \mid Zn$$

from the zinc sulphate to the water.

9. $a_1 + b_1 = E_1$, observed 90·2, calculated 90·0.

$a_1^1 + b_1^1 = E_2$ „ 86·7 „ 87·2.

10. $E_1 - E_2\,(3{\cdot}5) = Zn \mid L_2 + L_2 \mid L_1 + L_1 \mid (3{\cdot}9)$. From this follows that the law of electro-motive series holds for solutions of zinc and copper sulphates.

11. The potential of the combination

$$Zn \mid Zn\,SO_4 + Zn\,SO_4 \mid Cu\,SO_4 + Cu\,SO_4 \mid Cu$$

increases on diluting the zinc sulphate.

R. CLAUSIUS—ON SOME REMARKS OF C. NEUMANN ON ELECTRO-DYNAMICS.

(*Annalen der Physik und Chemie, B. XII., H. 4, No. 4*, 1881, pp. 639-643.)

In the introduction to a publication in the *Mathematischen Annalen*, Neumann mentions that Riemann, in order to deduce the explanation of electrodynamic forces from the already known forces of electrostatics, has made the assumption "that the potential of these forces, just like light, propagates itself with a certain constant velocity through space." The author has investigated the point, whether this explanation of the electro-dynamic forces can really be brought to an issue; and, from calculations he has made, he arrives at other formulæ which do not represent the electro-dynamic forces. The result of these calculations he sums up: "From this it appears to me undoubtedly to follow, that the assumption of Neumann (given above), by means of which he obtains his formula for potential, cannot be considered as correct on all points: whether the same formula may not be arrived at by other hypothesis as to the mode of propagation of the potential, cannot yet be decided." Neumann raises no objection to the author's mathematical theory, but withdraws part of his statement, since he says that the comparison with light was only accidental, and not essential. In place of the above mode of motion, which is an absolute motion in space, he puts a relative motion, which he defines in these terms: "The radius vector, which is the path of the potential, is itself in motion." After this alteration, which is in the direction the author had pointed out in his criticism, Neumann has stated that he cannot agree with the latter. As therefore some doubt might arise in the mind of a reader of Neumann's article, whether Clausius had not advanced incorrect statements, he feels himself called upon to reassert what he formerly stated.

In conclusion, he offers a few words on the influence of this explanation on the hypothesis. If the electro-dynamic forces could be deduced from electrostatic potential, through the hypothesis that the path of electricity through space is similar to that of light, a supposition first brought forward by Gauss in a letter to Weber in the year 1845, an advance of great importance would be made, since a deeper insight would be obtained into the nature of electricity, and its connection with light would be still further confirmed. But if a mode of motion has to be assumed which differs from the simple wave motion of light, in order to obtain coincidence with the electro-dynamic formulæ,—such assumption having no other ground than that it does show such a coincidence,—the hypothesis assumes quite another character, and the same physical probability can no longer be ascribed to it as in the former case. Neumann has also said that he, independently of Weber and Riemann, has assisted in introducing the conception of electro-dynamic potential into electricity, and he complains that the author in one of his publications takes no notice of his work. The latter explains that he wished to compare the formula he had himself obtained with those formerly obtained; and since Neumann's formula is exactly similar to that of Weber, which certainly had the priority, while Riemann's is different, he mentioned these two by name, while not speaking of Neumann.

W. HOLTZ—ON AN ARTIFICIALLY MADE BODY WHICH SHOWS POLAR ATTRACTION AND REPULSION.

(*Annalen der Physik und Chemie, B. XII., H. 3, No. 3*, 1881, *pp.* 477-480.)

The author thought that the so-called unipolar conductivity of certain bodies, which has lately been denied, might, after all, have an existence, and be caused by the fact that the molecules of such bodies, unequally arranged in different directions, or unequally constituted, experience a certain direction by the action of electricity at a distance (electrical far-working), in consequence of which they might oppose a greater resistance to the passage of electricity of one sign, and would give appearances of polar attraction. To test this experimentally, the author constructed the following apparatus:—He cemented a plane piece of glass on to one end of a short glass rod, and on to the plane of glass a short, very narrow glass tube in a line with the rod, in such a way that the end of the tube was closed by the glass plate, and not by the cement. In the small tube he placed a sewing needle, which projected slightly beyond the tube, and had stuck on it a thin cardboard disc (22 mm. in diameter) which was free to revolve. One-half of the periphery had a strip of cardboard glued on to it, about 10 mm. wide, and passing beyond the upper and lower surfaces. Opposite to the cardboard strip on one surface of the disc was fastened a small projecting point of tinfoil. Hollow discs, 100 mm. in diameter, were placed as far apart as possible on the discharge rods of a Holtz machine, and the apparatus brought between them, so that the axis of the small disc was exactly at right angles. On working the machine and reversing its poles, it was found that the tinfoil point always turned to the positive pole.

The author then hung a light glass tube about 300 mm. long in a loop of cardboard by two parallel cocoon fibres, about 40 mm. apart. To one end of this tube the apparatus was cemented, so that the little disc could not only rotate, but was capable of lateral motion. On bringing the apparatus between the two hollow discs on the machine, and working the latter, the disc first placed itself in position, and was then attracted to the negative pole. It did not matter whether the tinfoil point was directed forwards or backwards. It has long been known that particularly formed bodies, if directed in a particular way, tend more towards one electrode than the other. The so-called gold fish depends on this fact. Still more simple is a cork disc, one of the surfaces of which is furnished with a needle. It sticks fast if pressed against one of the metallic plates, because it loses more electricity by discharge than it receives by conduction. The above body also adheres to that electrode to which it is attracted, and it might not be unlikely that many substances which have distinct polarity might show a similar behaviour. It might be possible to employ such a body to show the direction of the current, at any rate for the electricity produced by machines.

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An Extraordinary General Meeting of the Society was held on Thursday, September 22nd, at 8.30 p.m., in the Electrical Exhibition, Palais de l'Industrie, Paris—Professor G. C. Foster, F.R.S., President, in the Chair.

The Secretary announced the names of the new candidates approved by the Council for ballot, and that the following Associates,

H. Edmunds, junior,
E. W. Parsone,

had been transferred to the class of Members.

M. le President: M. le Ministre, avant de se livrer à l'examen des matières scientifiques qui vont occuper la séance que votre bienveillance a permis à la Société des Ingénieurs Télégraphistes et Électriciens de tenir ici ce soir, nous avons le devoir et le plaisir de profiter de cette occasion pour vous offrir leurs félicitations les plus sincères au sujet de la collection vraiment remarquable d'instruments électriques qui se trouvent réunis en ce moment dans le Palais de l'Industrie.

En même temps, ils désirent ajouter à ces félicitations l'expression de leurs remerciements les plus empressés pour le bienveillant accueil qu'ils ont reçu de vous aussi bien que de la part de M. le Commissaire-Général de l'Exposition, M. Berger, qui

a si puissamment contribué à la réalisation de la bien heureuse idée d'exposition internationale d'électricité.

Comme témoignage de l'appréciation profonde des beinfaits pour la science d'électricité qui ne pourront pas manquer de naître de l'exposition par suite de la directions à la fois énergique et savante qui a présidé à un si beau succès, le Conseil de la Société des Ingénieurs Télégraphistes et Électriciens m'a autorisé à vous prier, M. le Ministre, de bien vouloir lui permettre d'inscrire votre nom sur la liste des membres honoraires de la Société. La présence de ce nom comme le cinquième dans la liste des membres honoraires sera pour nous un souvenir des plus agréables de cette mémorable séance.

M. le Ministre : Je voudrais dire à la Société qu'elle n'a pas à nous remercier ; c'est à nous, au contraire, à lui témoigner notre gratitude pour le grand honneur qu'elle a bien voulu nous faire en acceptant de tenir sa séance dans le sein même de notre Palais de l'Industrie. Je lui en témoigne toute notre reconnaissance, et je comprends parfaitement qu'elle s'est rendue compte du but que nous nous sommes proposés en organisant cette grande exposition et en organisant, en ouvrant surtout, le Congrès Électrique.

Vous avez parfaitement compris que ce que nous voulions, c'était de constituer au milieu de toutes les nations une grande famille scientifique, qui s'acharnat uniquement à développer la science et à augmenter les produits de l'industrie. Je vous en témoigne au nom de la France toute notre reconnaissance, et vous nous trouverez toujours dévoués à marcher dans cette voie.

M. Berger (Commissaire-Général de l'Exposition) : Je tiens à remercier l'honorable professeur Foster d'avoir bien voulu m'associer aux éloges qu'il à décernés à M. le Ministre. Si nous n'avons pas autant de mérite que M. Foster a bien voulu le dire, nous avons su profiter du bonheur rare que nous avons eu de trouver dans tous les pays des collaborateurs éminents et disposés à reconnaitre le but que s'était proposé mon pays. Sans la Société des Ingénieurs Télégraphistes, l'exposition ne serait pas ce qu'elle est, c'est-à-dire que la participation anglaise n'aurait pas été aussi complète.

À notre premier appel, votre Société, Messieurs, a tendu la main à l'administration française; elle a provoqué en Angleterre le grand mouvement qui a décidé le gouvernement de sa Majesté Britannique à mettre à la tête du commissariat anglais des hommes tels que lord Crawford, que Sir Charles Bright, que le professeur Hughes, mon ancien ami de 1878, le colonel Webber qui honorent leur pays et le pays où ils se rendent pour honorer la science.

Je serais injuste si je ne çitais pas içi le nom de M. Aylmer, qui à été pour nous le collaborateur dès la première heure. On peut dire que M. Aylmer, votre sympathique secrétaire parisien à été la cheville ouvrière de l'Exposition anglaise dès la première heure; il à été le trait d'union nécessaire entre les exposants anglais et l'administration française, et ce n'est que justice de prononcer son nom, en rendant à votre société l'hommage de reconnaissance que je lui dois personnellement.

The PRESIDENT announced that the first Paper to be taken was a *résumé* of the earlier days of Electric Telegraphy, by Mr. Willoughby Smith.

Mr. ALYMER: Messieurs,—M. Willoughby Smith me prie d'exprimer ses regrets de ne pouvoir pas prendre la parole en français. Il me charge de vous dire qu' il avait écrit son rapport dans l'idée qu'il n'y avait eu qu'un nombre restreint de rapports à être soumis à la Société ce soir; mais, comme le temps dont nous disposons est limité, et comme plusieurs de nos confrères français ont promis de nous lire des rapports très interessants, M. Smith croit qu'il vous sera plus agréable de les écouter, et il vous propose de vouloir bien considérer son rapport comme ayant été lu.

À la fin de cette séance, des exemplaires de ce travail, écrit en français et en anglais, seront distribués aux membres du Congrés qui nous ont honorés de leur présence. Les membres de notre Société recevront communication du rapport par la voie ordinaire du compte rendu publié dans notre Journal.

A RÉSUMÉ OF THE EARLIER DAYS OF ELECTRIC TELEGRAPHY.

By WILLOUGHBY SMITH (Vice-President).

When it was finally settled that we should hold an Extraordinary General Meeting in Paris on the occasion of the Exposition Internationale d'Électricité, it occurred to me that it would be a fitting time, whilst in the midst of such a wonderful collection of electrical appliances gathered under one roof, for us to pause, look back, and contemplate, for the mutual instruction of all who chose to avail themselves of the opportunity, the way in which such great and successful results have been achieved. Consequently my purpose in this necessarily short paper is to pass in brief review some of the most important incidents, many of which have come under my own personal experience, in connection with the earlier days of Electric Telegraphy. Of all the modern applications of electricity, one of the most important is its adaptation to telegraphic purposes.

No account of a practical electric telegraph had been published prior to the date of Messrs. Cooke and Wheatstone's patent of June, 1837. I say practical, because Ronalds, as early as 1823, published a description of an electrical telegraph of originality and merit; but as he proposed to use static electricity in connection with a wire enclosed in a glass tube, his telegraph was not adapted for practical use.

In the September following the date of their patent, Messrs. Cooke and Wheatstone made their first practical experiment on the London and Birmingham Railway, and demonstrated beyond all doubt that as beacon fires and torches had been superseded in 1792 by the aërial or semaphore telegraph, so now the latter would have to succumb to a far more important rival. But incredible as it may appear at the present time, the contest was long and fierce, and not until 1852 did the final defeat arrive, as far as England was concerned, when the Electrical Telegraph superseded the Semaphore which had so long done duty between Liverpool and Holyhead.

On June 12th, 1837, Messrs. Cooke and Wheatstone patented "improvements in giving signals and sounding alarums in distant places by means of electrical currents transmitted through metallic circuits." This patent included the five-needle instrument, requiring five line wires; a four-needle instrument on the same principle, also requiring five line wires; methods of insulating and supporting the line wires by covering them with cotton and varnish and placing them in a resinous cement, in troughs or tubes; and also methods for localising faults in the same.

On April 18th, 1838, Mr. Cooke obtained a patent, the title of which was precisely the same as the first patent, and contained no additional matter worthy of note.

On the 21st January, 1840, Messrs. Wheatstone and Cooke obtained a patent, the title of which was similar to the two already mentioned, and in it are described a signal apparatus by which the letters of the alphabet are presented at an opening in a dial-plate by means of an electro-magnet in the circuit, which acts upon the pallets of an escapement put in motion by an independent clock-work; an electric alarum; a magneto-electric machine to be used in connection with the apparatus above described, etc.

On the 8th September, 1842, Mr. Cooke obtained a patent for stretching, suspending, and insulating wires on posts erected at suitable distances in the manner now so familiar to all of us both across the country and parallel with railways. This patent also mentions that "the wires so stretched, suspended, and insulated, may be used for one half the circuit, in conjunction with the earth for the other half."

On the 6th May, 1845, Messrs. Wheatstone and Cooke secured a patent for "Improvements in Electrical Telegraphs, and in apparatus relating thereto, part of which improvements are applicable to other purposes." In this patent it is proposed to give audible signals at the same time that visible signals are made by a needle or pointer, a distinct and different sound being given for each direction or deflection of the pointer. This was to be done by either the pointer itself striking a bell, or by alarum mechanism. Also by using the "derived current," a short branch circuit, or what we should now term a shunt, taken from the main

wire without disturbing its continuity, to communicate to a sensitive signal apparatus the signals passing through the main wire. This patent includes eleven other suggestions or improvements, which I think we need not stop to investigate.

Who really invented the Electric Telegraph is a problem as impossible to solve definitely, as who invented railways or steam navigation. I therefore mention these patents simply because, soon after the date of the last named, the first Electric Telegraph Company in England was formed for the purpose of working them more for the general use of the public than Messrs. Cooke and Wheatstone had hitherto done, they having principally confined their attention to applying the telegraph to the use of railway companies as an adjunct to their several systems of signalling.

Although one could loiter and note many points of interest which occurred in the history of Electric Telegraphy from its commencement in 1845 to 1850, I fear time will not allow of our doing so; let us therefore pass to the year 1850, and observe what progress had been made to that date. In England the "Electric Telegraph Company" had extended their wires to 2,215 miles, and were using double and single needle instruments, an important modification of the five and four needle instruments patented by Messrs. Cooke and Wheatstone in 1837. In Ireland they had not yet realised the value of the Electric Telegraph, for although they had 500 miles of railway, they had not five miles of telegraph wire. But in America they had 12,000 miles of wire, and were using the Morse, Bain, and House instruments, all on the permanent recording principle. The only reasons I can assign for the more rapid progress which had been made in America, are their low charges and rivalry resulting from competition, as there were no less than twenty different Companies at work, who did not confine their lines to the railways, but erected them on public roads or private property as occasion required; whilst in England one Company had the monopoly, and erected their wires on the property of the Railway Companies only; but as there were 5,447 miles of railway open at the time, their powers of extension were not limited in that direction. In France there were only 620 miles of wire in operation, suspended similarly to those in England, the

instruments used being what were termed revolving pointers. In Prussia they had 2,468 miles of wire, mostly subterranean, and similar instruments were used to those worked by the French Government.

In America they were also at this date employing the Electric Telegraph in their meteorological observations, and had commenced a system of storm warnings along many parts of their coast, similar to that we are now so familiar with in England. It has been said that to predict an eclipse is an object merely of curiosity, but to predict an approaching storm would be of inconceivable benefit. It is true we have a considerable acquaintance with the nature of heat, water, air, and electricity; but when these four ingredients of nature, in a compounded state, are floating round our globe, and producing all those various agitations and combinations known under the general denomination of "weather," we are puzzled and perplexed, and although Electric Telegraphy has done much to enlighten us, still there is much more to be learnt. No doubt if the information which is being daily recorded by patient observers in all parts of the civilised world were readily and quickly communicated to one centre, great advancement in our meteorological knowledge would soon follow, and I hope the day is not far distant when such a system will be an accomplished fact.

Electric Telegraphy having made such satisfactory progress, it became absolutely necessary that England (the island centre of the commerce of the world) should no longer remain in her isolated position, but be put into direct telegraphic communication with other nations. Consequently, in 1850, a company was formed in connection with the concession just obtained from the French Government by Mr. Brett for the sole right to establish telegraphic communication between England and France. Mr. Brett had guaranteed that with his instrument, which was a modification of House's step-by-step-motion printer, one wire and two persons only, the one stationed in France and the other in England, one hundred messages of fifteen words each, printed in clear Roman type, should be sent ready for delivery in one hundred consecutive minutes. That was a bold undertaking on the part of Mr. Brett, considering the various opinions held in those days as

to the practicability of ever being able to either lay or work a submarine line.

It was arranged that 25 miles of copper wire ·083 of an inch in diameter should be covered with gutta percha (at that time a material of comparatively recent importation, the mechanical and electrical properties of which were just developing themselves) to half-an-inch in diameter. The copper was unannealed, and covered to the full gauge with one covering of gutta percha, in 100-yard lengths. The copper was joined by what is termed a bell-hanger's twist, and then further secured with soft solder. Gutta percha in a plastic state was then applied and pressed into shape in a wooden mould, the diameter of the finished joint being about two inches. Of course in those days the joints were not tested separately, nor was any notice taken of the resistance or conductivity of the copper, or the resistance or electrostatic capacity of the gutta percha. The coils, when joined into suitable lengths for carriage, were simply immersed in water, at no specific temperature, and if no serious deflection was obtained on a very sluggish vertical galvanometer from 24 cells of the old form of sand battery, they were pronounced good, and joined to the length which was being coiled on to a large iron reel constructed to hold the 25 miles in one continuous length. This drum or reel was placed amidships on the deck of a steam-tug. Owing to the great number and size of the joints, to place the wire on the reel with the required uniformity the inequalities were filled in with cotton waste, and when thought necessary thin wooden laths were laid longitudinally the whole length of the reel, between the layers of the wire.

Previous to the submersion of the main wire the shore ends were laid, one from a horse-box, which was to serve as the shore station, in the yard of the Railway Company at Dover, to the far end of the staging erected in connection with the new harbour which was then being constructed. The other was laid a considerable distance beyond low water mark, over the rocks into the lighthouse on Cape Grisnez. These shore ends consisted of a copper wire ·065 diameter covered only with cotton and a solution of india-rubber, and enclosed in a very thick tube of lead. Early

on the morning of the 23rd August, 1850, the tug steamed out of Dover harbour to the end of the shore length. The end of the main wire was then passed into a boat where it was joined to the shore end, and the laying of the main wire was commenced, H.M.S. "Widgeon" leading the way. As every 110 yards left the tug a leaden weight was attached to the wire, varying from 14 to 24 lbs., according to the depth of water or nature of the bottom. At first it was attempted to fix the weights without easing the speed of the tug or the egress of the wire, but as each weight was in two halves, and had to be placed on the wire, and then fastened by hammering the two studs of the bottom half which came through corresponding holes in the top half, it was soon found that the two men so employed struck each other more often than they did the desired object, consequently it was found necessary to stop the vessel while each weight was being attached. The weather was all that could have been desired, and with the exception of the confusion caused at times by the lashing effect of the wooden laths as they became partially released from the drum, all went well, and at six the same evening the anchor was dropped from the steam-tug close under the inhospitable-looking shore of Cape Grisnez, and at the position of the buoy attached to the shore end. The end of the main wire was then passed into the cabin, where it was connected to the printing instrument. But it was soon apparent that either the orthography of the sender at Dover was very far from correct, or that the instrument was not working as it was contemplated it would. Letters in "bold Roman type" were certainly received, but it required great ingenuity to arrange them in their proper order. As the evening was fast approaching, too much time could not be given to the experiments on board, consequently the end of the main wire had to be passed into a boat in which the shore end had previously been secured; there the splice was made and then passed overboard with the sincere wish that the communication that evening effected between England and France might serve to still further establish the two nations in unity and love. All then landed and made the best of their way to the lighthouse on the top of Cape Grisnez, where a single-needle instrument was placed in circuit with Dover, but although

many anxious hours were passed in endeavouring to get signals from Dover, none came. The trials of the next day were equally unsuccessful, and then it became apparent that the line must have broken down; and as all the attempts made to lift the wire at or near the splice failed, the line was eventually abandoned. Had the main wire been used instead of the heavy lead-covered shore end, I believe better results would have followed. But criticism can now only be made through the light of experience, which would not be just. Let us rather award all honour to those gentlemen who devoted their money, time, and energy to a practical demonstration of the feasibility of submarine telegraphy. From that moment it became simply a question as to the best way to protect the conductor and its insulator during the laying and after submersion.

After careful consideration, it was, in 1851, finally agreed that instead of one wire of the size used in the experimental line of the previous year, four copper wires ·065 diameter, covered separately with two coverings of gutta percha to ·25 inch diameter, should be twisted together and well protected with thick iron wires laid spirally around them. Yarns saturated with tar were laid in the interstices of the twisted conductors, and designated the "worming," and the whole covered with similar yarns laid on at right angles and designated the "serving." Twisting the gutta percha wires, the worming, and the serving, were all done at one operation in one machine, and the wire thus covered was designated the "core." The whole was then passed through the lay-plate of an ordinary wire-rope-twisting machine, where ten galvanised iron wires ·25 diameter were laid round it, and as it passed from the machine it was coiled in the factory yard. The copper conductors were insulated by the Gutta Percha Co., and forwarded in suitable lengths to a small wire-rope manufactory in High Street, Wapping, where it was twisted, wormed, served, and covered with iron wires as described. Owing to litigation as to patent rights, the manufacture of the cable was seriously delayed, and not finally completed until the 17th September, 1851. Preparations were at once made to coil the cable in one length on board the "Blazer," a hulk lent by H.M. Government, and on the

24th September all was coiled, and the "Blazer," in charge of two steam-tugs, started for the South Foreland, where on the following day one end of the cable was landed and the laying commenced, H.M.S. "Fearless" showing the course, and the "Blazer" being towed by two tugs, with a third tug in attendance. To enumerate the many exciting incidents which occurred on board the "Blazer" that day would occupy too much of our time. Owing to the unfortunate delays during the manufacture of the cable, the laying was obliged to be undertaken, to save the concession, during the prevalence of equinoctial gales and the period of the strongest tides. Although the weather was fine at starting, it soon changed, and before noon it was blowing a gale, and the "Blazer," helpless old hulk as she was, rolled and pitched most unmercifully. The contrivances employed to retard the egress of such a heavy cable were insufficient, consequently during the two stoppages which occurred—one, when from some cause on shore there was a total cessation of signals, and the other from the breaking of the tow-rope—much cable was wasted, and eventually the expedition arrived off Sangatte late in the evening, with insufficient cable to reach the shore. Never had there been such a continuous struggle between mind and matter as there had been the whole of that eventful day, and it was to be regretted that matter for the time gained the victory. But nothing daunted, a sufficient length of cable to reach the shore was at once ordered; and in the meantime three wires were twisted together, and, without any protection, laid to the beach, and there connected to the wires already laid to a room at the railway station at Calais. Thus on the 30th September, 1851 (or within a few days of thirty years ago), was telegraphic communication established between England and France. On the 19th October the additional length of new cable replaced the three wires, and on the 13th November the cable was first used for the transmission of public messages, and with slight interruptions has continued to do so to the present time.

The possibility of submarine telegraphy having been so satisfactorily demonstrated, it was but natural that many and various schemes should have been immediately devised for its further development. But, unfortunately, owing to the great haste and

total disregard of even ordinary precautions, some of the schemes proved lamentable failures. For instance, in the June of the following year an attempt was made to connect England and Ireland by means of a line from Holyhead to Howth. In this instance one gutta percha covered copper wire, similar to those in the Dover and Calais cable, was covered with No. 8 iron wire for shore ends, then with twelve No. 14 iron wires for a few miles, but the greater portion of the core was only partially covered, bird-cage fashion, with six No. 14 galvanised iron wires, consequently through the core having no serving or hemp covering, the gutta percha was much exposed. When completed it was coiled on trucks and taken by railway to Whitehaven; but, as the steamer which was to receive it could not enter the harbour there, it had to be re-coiled and forwarded to Maryport, where it was coiled on board a paddle steamer and taken to Holyhead. There it was electrically tested for the first time, and found to be faulty. As it had never been immersed in water, and was then perfectly dry in the holds of the steamer, of course only faults of a very serious character could be detected and rectified, which was done at the cost of much time and trouble. Had it been covered with water, its imperfect electrical condition would have been discovered, and most likely it would not have been laid at all. All attempts to repair this line after it was laid proved fruitless, for the simple reason that the necessary strain to lift it drew the iron wires together, cutting through the gutta porcha as they forced the core from the centre.

Later in the same year an attempt was made to lay from Portpatrick to Donaghadee a three-strand hemp rope about two inches in diameter, in the interstices of which was laid a copper wire which had first been covered with india-rubber, and then with gutta percha. But, owing to the inadequate means provided for sinking such a rope, the strong currents carried it far from the proposed course, and eventually the attempt was abandoned.

Later still in the same year another attempt was made between the same points, with a cable similar in construction to that laid in the previous year across the English Channel, but owing to unfavourable weather and inadequate means for stopping,

or even retarding, the egress of such a heavy cable, the length provided proved too short, and eventually this expedition returned also unsuccessful. Thus had three cables been lost in attempting to electrically connect England and Ireland; but in the following year a heavy multiple cable was successfully laid from Portpatrick to Donaghadee. Four light cables were also laid in that year between England and Holland, and a heavy multiple one between Dover and Ostend.

The year 1853 was also notable for the large amount of subterranean wires laid in England by several of the then existing telegraph companies; but as I went somewhat into detail on that subject in my paper on "Underground Wires," which I read before the members of this Society on the 28th March, 1877, and which was published in Vol. VI. of the Journal of the Society's Proceedings, it is unnecessary for us to go over the same ground again.

From 1851 to 1856 there had been successfully laid, not in connection with England only, but in various parts of the world, about 1,600 miles of cable, and it was thought that sufficient experience had been obtained to bring to a successful issue the long-cherished scheme of laying a cable from Ireland to Newfoundland, and thus bring America into direct telegraphic communication with England. Soundings were taken, and it was asserted that although rough and jagged rocks did project high above the bottom, still there was a level plateau, or smooth path, between the two islands, as though nature, anticipating the wants of man, had prepared a resting place for telegraph cables, where, if properly placed, they would lie in perfect peace, and where the ooze, which it was alleged was constantly descending through the water, resembling the fall of snow through our atmosphere, would effectually cover and protect the cable to the end of time.

Experiments were made with long lengths of submarine and subterranean wires, and the results stated to be highly promising as to a remunerative speed being obtained through an Atlantic cable 2,000 miles in length, using a comparatively small conductor. The mechanical properties of various specimens of what were considered suitable cables were fully tested, and finally the

choice fell on one in which the conductor consisted of a seven-wire strand of copper, weighing 107 lbs. per knot, trebly covered with gutta percha, the weight of which was 261 lbs. per knot, the external diameter being $\frac{3}{8}$ of an inch. Around the gutta percha were spirally laid hemp yarns saturated with a composition of tar, pitch, and tallow, over which were also spirally laid eighteen iron wire strands, each strand being composed of seven wires of the same diameter as those used in the conductor. The weight in air of the completed cable was 20 cwt., and in water 13·4 cwt. per knot, its breaking strain being about 3·25 tons. The final selection of this cable was not made until late in the year 1856, and, as it was stipulated that 2,500 miles must be shipped and ready for laying in the June of the following year, it allowed only six months for all the work to be done. In that short space of time 116 tons of copper had to be prepared and drawn into 17,500 miles of wire, which had to be twisted into 2,500 miles of strand; 250 tons of gutta percha had to be what is termed "manufactured," and placed around the conductor in three separate coverings; 1,687 tons of charcoal iron had to be specially prepared and drawn into 315,000 miles of wire, from which 45,000 miles of strand had to be twisted, and then laid around the core. Add to all this the fact that ships had to be selected and got ready to receive the cable, machines both for manufacturing and laying had to be constructed, and no surprise need be expressed that experience soon taught that so short a time was totally inadequate for such an important work to be executed in a proper and efficient manner.

Lengths of the core varying from 1,600 to 2,400 yards were immersed in water at no fixed temperature, and shere tested for insulation with 504 cells of the old form of sand battery, and a horizontal galvanometer. If the deflections of the coils of each batch were constantly uniform, their electrical condition was certified, and each coil, wound on a wooden reel, was forwarded to the cable factory.

Soon after the commencement of the manufacture of the cable, frequent want of continuity in the conductor occurred through imperfections in the copper, from improper annealing and other

causes. Unfortunately several hundred miles of the core were manufactured before the machines were ready to cover the same with the serving and iron strands; the fatal edict therefore went forth that all the core in stock should be submitted to tensile strain as it passed over a barrel of small diameter, to develop, if possible, any defects in the conductor before it passed the wire-covering machine. Now when a wire or strand of copper covered with gutta percha is stretched, the metal becomes permanently elongated and holds the elastic material in that position until a favourable opportunity occurs for the gutta percha to return to its normal state, distorting the wire in so doing. Unfortunately it was not long before such an opportunity occurred; the cable as manufactured was coiled, not in tanks where it could be kept under water, but where it was exposed to the powerful effects of the sun's rays. The consequence was that the gutta percha became plastic in parts, and the conductor, being forced out, came into direct contact with the serving, which, being dry, acted the part of an insulator, and thus prevented detection while the cable was in a dry state. It was to be regretted that the manufacture of the cable was too far advanced for advantage to be taken of Sir William Thomson's discovery of the great variation in the electrical conductivity of commercial copper, especially in that then being used for the conductor of the Atlantic cable. The importance of that discovery has since been fully realised, not only in every branch of electrical science, but by also enabling a purer copper to be obtained, for whatever purpose required; for, as is now generally known, the higher its electrical conductivity the purer the copper.

It was not until the 5th of August that one end of the cable was landed at Valentia and the laying of the cable commenced; but soon after starting, difficulties began to manifest themselves, and when 335 miles were laid the cable broke just astern of the paying-out machine, and the expedition was abandoned for that year. The remaining portion of the cable was coiled in the dock-yard at Plymouth, where it was overhauled, but not immersed, and passed through a hot insulating mixture, then spliced to 700 miles of new cable, making the total length about 3,000 miles.

In the following June, the cable having been re-coiled, half on

board the "Niagara" and half on board the "Agamemnon," various experiments were made in the Bay of Biscay, the results of which were considered satisfactory to the proposal of splicing the cable in mid-ocean and both ships paying out cable at the same time, one towards Ireland and the other towards Newfoundland. On the 26th of June the splice was made in mid-ocean, but after paying out three miles from each ship the cable broke at the stern of the "Niagara." The ships at once returned to their former positions and re-spliced, but when about forty miles had been laid from each ship a total cessation of signals occurred, and those on board each ship, supposing an accident had happened on the other (an incident which shows one of the disadvantages of this mode of laying cables), they each returned to the position where the splice had been made, and on meeting anxiously inquired one of the other, "How did the cable part?" The cause of rupture this time proved to be a profound mystery, for all was proceeding satisfactorily on both ships when the apparent want of continuity occurred. The cable was again spliced, and about 200 miles laid when it broke close to the stern of the "Agamemnon," and the ships returned to Ireland.

On the 29th July they returned again to mid-ocean and spliced the cable there once more, but had not proceeded far in paying out before there was reported a want of continuity. Fortunately they continued to pay out slowly from both ships, and after two hours' anxious suspense the signals reappeared as mysteriously as they had previously disappeared. Such incidents frequently occurred again, but no actual rupture took place, and on the 5th August each ship had completed its task, the "Niagara" having laid 1,030, and the "Agamemnon" 1,020 knots. It was true the cable was laid, but its electrical state must have been in a most deplorable condition, for nothing but what was analogous to brute force could produce spasmodic, emaciated shadows of the intended signals, and in twenty-four days it was unable to produce those results, the whole being pronounced a failure. It was a failure in one sense of the term, but it should not be forgotten that much valuable experience had been obtained, and that the problem was solved as to the feasibility of laying and working an Atlantic cable.

About this period, time began to develop the fact that the system of using such a strong solvent of gutta percha as coal-tar naphtha between each separate covering, with a view to causing more perfect adhesion, had very prejudicial effects on the gutta percha, consequently I began to experiment with a view to obtaining an adhesive solution or compound which would in no way injuriously affect the gutta percha, and soon found that suitable proportions of Stockholm tar, resin, and gutta percha produced a compound suitable for the purpose. Not only did this compound possess good adhesive qualities, but it was also a good insulator, and when applied between each alternate covering of gutta percha, improved the insulation threefold. This compound has done and is still doing service, and its qualities are so well known that no further notice need be taken of it here.

It was about this time a cause for surprise and regret that the English Government should have financially connected itself with such a gigantic scheme as that of laying a cable from Egypt to India while the success or failure of the Atlantic cable was still in the balance. Had they awaited the result of the Atlantic expedition, perhaps the lamentable failure which so soon followed might have been avoided. The cable of the Red Sea and Indian Telegraph Co. consisted of a core containing 180 lbs. of copper, in the form of a seven-wire strand doubly covered with 212 lbs. of gutta percha per mile, served with tarred yarns and covered with eighteen iron wires. The gross weight per knot was about twenty-one cwt. It was laid in six sections, from Suez to Kurrachee. To give the history of the manufacture, laying, and ultimate failure in working, would only be to repeat, with unsatisfactory additions, what has already been said concerning the Atlantic cable. Suffice it to say that the 4,000 miles were manufactured and laid by the contractor in the short space of fifteen months, and during the same time the same contractor was experimenting in laying hemp-covered cables in the Levant, where about 800 miles were lost.

These serious failures, involving as they did the loss of about 8,000 miles of cable, in so short a time, naturally suggested a thorough investigation of the whole matter; therefore a joint-committee, appointed by the Lords of the Committee of Privy

Council for Trade and the Atlantic Telegraph Co., was formed to inquire into the construction of submarine cables; and I believe the arduous labours of that committee did much for the ultimate success of submarine telegraphy.

In June, 1859, the English Government ordered 1,200 nautical miles of cable to be laid between Falmouth and Gibraltar. Engineers and electricians were appointed to inspect the manufacture of this cable. The core consisted of equal weights of a seven-wire copper strand trebly covered with gutta percha and compound, weighing altogether 800 lbs. per mile. This core was covered with tarred yarns and solid iron wires in the ordinary way. Instruments were practically employed for measuring the electrical qualities of the copper and gutta percha at a uniform temperature, and the results were recorded in electrical units. That is to say, the specific resistance of the copper and gutta percha was so noted that the measurements would be comparable with the results obtained during manufacture and after submersion. The core in suitable lengths was submitted to a pressure of 600 lbs. to the square inch, and it was invariably found that the resistance of the gutta percha increased when so circumstanced. The gutta percha on one of the coils having been by accident pierced through to the conductor while placing it in the pressure tank, it was found that the water was forced along the strand and out of the ends which were brought through stuffing-boxes for testing purposes. To obviate this in future, I had the centre wire of the strand passed through the compound before the other six wires were laid around it. This had the desired effect, as water at a pressure of 1,000 lbs. per square inch could not be forced through six inches of a core containing a strand so constructed. This cable was also coiled into water-tanks as manufactured; but the ships engaged to lay it were not fitted with tanks, consequently soon after shipment of the first section an increase in the resistance of the conductor denoted an increase in the temperature of the cable, which was found to arise from fermentation, but whether caused by the hemp serving or the oxidation of the iron, opinions differed. By thermo-electric measurements it was found that the temperature varied from 84 to 62° Fahr. in different parts of the same coil, while the external temperature

kept uniform at 60°. It was thought that the high temperature might have permanently injured the gutta percha, but a length of core taken from the hottest part showed no signs of deterioration.

Owing to the delay of the ships the season was too far advanced to lay it where first intended; it was therefore arranged to lay it between Rangoon and Singapore. But the ship carrying the first portion came to grief in the English Channel, and eventually it was laid in three sections between Malta and Alexandria, where, owing to the shallowness of the water in which parts of it were laid, and the light character of its construction, it was frequently out of order, and had to be superseded by a cable which was laid direct from Malta to Alexandria in 1868.

In December, 1860, I first called attention to the fact that mechanical injuries to the core of a cable, from accident or otherwise, were often concealed by the insulating properties of the tarred serving, and that according to the nature of the fault and the amount of tar so was the period of the concealment—a fact sufficient to account for the failure of insulation of so many cables after they have been laid in apparently perfect condition. To obviate this I advised that the serving should be saturated with a conducting instead of an insulating preservative fluid, and that the core should be always kept in water and passed direct from the same to the iron-covering machine, where the presence of the wires at the lay plate would compress the serving, and force the fluid into any defect in the gutta percha, and immediately develop the same. The suggestion was readily adopted, and soon proved effective; since then tarred serving has been the exception, not the rule.

On the completion of the first Malta and Alexandria cable, the Government ordered one to be laid in the Persian Gulf; but before deciding on the form of cable, a great many experiments were made, especially on the electrical and mechanical properties of various cores consisting of gutta percha or india-rubber, pure and simple, or compounded with other substances. Eventually the one selected was to consist of a segmental copper wire, weighing 225 lbs. per knot, insulated by four coverings of gutta percha, with the compound already referred to applied in the ordinary way,

weighing 275 lbs. per knot, the total weight of the core being 500 lbs. per knot. A strand and solid wire, of the same copper and of equal weights, under the same conditions, conduct precisely the same; but in lateral induction there would be a slight disadvantage in using a strand, and it was therefore to obtain the mechanical advantages of the strand and the electrical advantages of a solid that a segmental wire was proposed, which was constructed of four copper wires, each forming the segment of a circle, drawn into one copper tube; but the mechanical difficulties in the manufacture were so great that it was abandoned for a solid wire before the completion of the cable. The electrical qualities of the core were tested in suitable lengths at a uniform temperature of 75° Fahr., while under a pressure of twenty atmospheres per square inch, and the electrical resistance of the gutta percha in-invariably increased about 40 per cent. under such conditions, but returned to its normal resistance when the pressure was removed.

The now well-known accumulation method was for the first time applied to test the joints in this core. The serving was saturated with tan water instead of tar, tallow, etc., as heretofore used, and the served core was coiled in water-tanks. Twelve iron wires of the required diameter to envelop the serving when laid helically around it were then covered with two layers of yarns wound in opposite directions.

A hot bituminous compound, composed of Stockholm tar, ground silica, and pitch, was applied with each covering of yarn; the whole passing through powerful compressed grooved rollers, which gave it a smooth black surface. As manufactured, it was coiled into water-tight tanks, and under the same conditions the 1,450 knots were transported in four sailing vessels and laid in four sections between Kurrachee and Faa, the intermediate stations being Gwadir, Mussendom, and Bushire. The very satisfactory results on the completion of this cable certainly compensated for the great amount of care and attention which had been bestowed on its selection, manufacture, and laying.

During the time occupied in the manufacture and laying of the last-mentioned cable, two attempts to lay specially-constructed cables were made in the Mediterranean. It had been proved, by

a series of well-conducted experiments, that if hemp yarns were placed around an iron or steel wire at a certain angle, and in certain proportions, the breaking strain of the two would be greater than the sum of their separate breaking strains; consequently an ordinary gutta percha core was surrounded with wires so covered with hemp, to be laid between Toulon and Algiers, but owing to the "springy" character of the cable, and the incompleteness of the paying-out arrangements, the cable broke while being laid, and, having to manufacture new cable to replace the portion lost, it took twelve months to complete the undertaking. The other experiment was with 130 knots of an ordinary gutta percha core, covered with yarns saturated with a conducting fluid, put on at a uniform tension, with a slight twist, and then covered with a continuous tubular metallic casing, which was applied by winding a flat ribbon of copper in a helical direction around it in such a manner that the edge of each convolution overlapped that of the preceding one. The whole length was coiled round a cone on a revolving disc, or turntable, fixed in the hold of a steamer, and attempts were made to submerge it between Oran and Carthagena, but its construction proved totally unsuited for the purpose, as did also the arrangements of the revolving disc or table, and the expedition was abandoned after having lost about half the cable.

Notwithstanding these and other failures, sufficient had been successfully accomplished to warrant another attempt to connect Ireland and Newfoundland, for during the five years which had elapsed since the last attempt great improvements had been made in every branch of submarine telegraphy. It was accordingly decided to manufacture 2,300 knots of cable, consisting of a core containing a seven-wire strand of copper weighing 300 lbs. per knot, covered with four coverings of gutta percha and four of compound, applied alternately to a diameter of ·464, and weighing 400 lbs. per knot, the total weight of the core being 700 lbs. per knot. The serving was ordinary hemp saturated with tan liquor. The external protection consisted of ten solid iron wires, each surrounded separately with Manilla yarns covered with a preservative compound of tar, india-rubber, and pitch. The whole cable was coiled in three separate tanks on board that marvellous conception

of Brunel, the "Great Eastern," and the expedition started in the summer of 1865. From the extraordinary care, even to the minutest detail, bestowed on all connected with this cable, it was believed that success would be the reward; but during the manufacture, coiling on board, and the laying, faults occurred in the insulation which, when found, suggested the horrible thought that the hands of some evil-disposed person or persons had been at work, as there was the undeniable fact that some blunt instrument had been forced between the wires of the outer covering, through the gutta percha to the conductor, and then withdrawn. One such fault occurred when seventy-nine knots had been laid, and ten knots had to be picked up to remove the same. Another similar fault developed itself when 750 knots had been submerged, and three knots had to be picked up from 2,050 fathoms to remove it. Another fault appeared in 2,200 fathoms, when 1,214 knots had been successfully laid; but this was never found, for after picking up three knots the cable parted, and the end of the sea portion was lost. After various unsuccessful attempts to recover it, the expedition returned home, the season being too far advanced to allow of any further attempts being made that year.

It was generally admitted that had not those mysterious faults occurred in the way and at the time they did, there would have been no difficulty in successfully laying the cable; and therefore it was decided that 1,600 knots of new cable should be manufactured, and in the following year (1866) another trial be made to connect Valentia and Newfoundland, and that then an attempt should be made to pick up the lost end of the previous year's cable, and complete the laying of that cable also. The 1,600 knots of new cable differed somewhat from the former one. The iron wires were more pliable, were galvanised, and no compound used on them or their jute covering.

The core of these cables was not submitted to pressure during the electrical tests, for in no instance had pressure developed faults; but actual experiments proved that faults which would be detected under pressure of one atmosphere became more or less concealed according to the amount of pressure applied. The results of an experiment with a faulty coil is given in the following

table, from which you will more easily see the proportion in which the resistance increased with an increase of pressure :—

1	atmosphere	=	49	megohms	per knot.
17·5	,,	=	51	,,	,,
35	,,	=	53	,,	,,
52·5	,,	=	54	,,	,,
70	,,	=	55	,,	,,
87·5	,,	=	57	,,	,,
105	,,	=	58	,,	,,
122·5	,,	=	58	,,	,,
140	,,	=	63	,,	,,
157·5	,,	=	64	,,	,,
175	,,	=	90	,,	,,
183·5	,,	=	115	,,	,,

On removal of the pressure the resistance suddenly increased to 178, then gradually began to fall, and at the end of seventeen hours had returned to its normal condition, or 49 megohms. If the pressure be allowed to decrease very gradually, it is very interesting to note the gradual increase in the resistance until the pressure is entirely removed, and then the resistance begins to decrease; but if, as stated, the pressure be suddenly removed, the resistance as suddenly increases.

Owing to the hardness of the iron wire used in the cable of 1865, its adhesive compound, and the long time it had been lying coiled in the tanks on board the "Great Eastern," several exciting scenes occurred while submerging that portion, from the layers of cable in the tank sticking together and becoming entangled, thus causing what are technically known as "foul-flakes;" but with these exceptions nothing could have progressed more favourably than did everything connected with the work; and after fourteen days and nights of care and anxiety, all engaged had the satisfaction of knowing that their labours had been crowned with success. After necessary preparations the expedition started to recover the lost end of the 1865 cable, and after twenty days and nights' hard struggle with the elements, the end was recovered. After speaking with Valentia and ascertaining the electrical condition of the

whole length to be perfect, the end was spliced to the cable on board, and paying out towards Newfoundland commenced. All proceeded well until within a few hours of completing the laying, when at six a.m., while receiving from Valentia a summary of the news from the *Times* of that morning, a sudden fault in insulation occurred. This was soon found to be on board, so the cable was cut and respliced, and to the credit of all be it recorded that in less than two hours the fault had been removed and paying out recommenced. At five p.m. the same day the laying of this cable was also successfully completed. It was fortunate that the fault of the morning had been detected before it passed out of the ship, for on careful examination it showed that one end of a broken wire in the top coil had by some means got bent at right angles and had entered the coil beneath, and the pressure caused by one of the tank men stepping on it forced the wire through the gutta percha to the conductor. As this fault was precisely of the same character, and occurred in a portion of the same cable, as those of the previous year, it was but fair to assume that they were caused in a similar way, and not maliciously, as at first supposed.

The particulars of my system for testing and working these cables, which I employed during the laying and after submersion, I gave in the paper which I read at this Society's meeting on the 12th February, 1879, and which is published in the Journal of our proceedings, and therefore I need not notice it further here. No doubt the laying of the one cable, and the recovery and completion of the other, were the crowning achievements in submarine telegraphy, and gave to that branch of our science an impetus that knew no bounds until all civilised countries were connected telegraphically as one nation. At the present time there are at work in all no less than 70,000 miles of submarine telegraph cables. Thirty-three years' experience has taught us that neither the electrical nor mechanical qualities of either gutta percha or copper deteriorate by long working or submersion, and also that the iron wires, when properly protected from oxidation, retain all their original qualities; consequently the best form of a submarine telegraph cable will be that in which these conditions are fulfilled.

The progress in the perfection of all instruments and appliances

connected with electric telegraphy is so well shown in the Exhibition which has brought us together here that I need not refer to them now. Although I have, I fear, encroached somewhat too long upon your time, I feel sure you will in conclusion allow me to embrace this fitting opportunity of acknowledging the uniform kindness I have always experienced in this country, whenever I have visited it upon either business or pleasure. My first visit was when I landed at Cape Grisnez on the completion of the experimental line in 1850, after an exciting day's work and eighteen hours' fasting. I have at the present time a vivid recollection of the kindness of the lighthouse keeper, who not only shared with me his frugal meal, but rendered all the assistance he could to my personal wants.

A vote of thanks having been accorded to Mr. Willoughby Smith, the President called on M. Mercadier for his paper.

RADIOPHONIE.

Par M. Mercadier (Foreign Member).

M. le Ministre, Messieurs,—J'ai d'abord à adresser un double remerciment à la Société des Ingénieurs électriciens de Londres, pour avoir bien voulu me demander: d'abord de prendre la parole dans une pareille assemblée, et, en second lieu, de traiter ici un sujet se rapportant directement aux derniers travaux que j'ai effectués, et qui sont relatifs à la radiophonie. Permettez moi de m'excuser d'avoir pris ce mot que j'ai proposé il y a quelque temps, et que je ne me serais pas permis de mettre en tête de cette conférence, s'il n'avait été accepté par les principales personnes qui se sont occupées de ce sujet; en particulier et tout d'abord par les inventeurs mêmes, MM. G. Bell et Tainter, et ensuite, par M. Preece, autrefois Président de cette Société.

Disposant de très peu de temps, et ne voulant pas abuser de celui qui m'est accordé, il m'a paru impossible de traiter ce sujet en le développant historiquement, ce serait trop long; il me suffira de dire que les faits dont je vais parler, en les résumant, résultent des travaux qui ont été faits simultanément ou presque simultanément dans plusieurs pays. Je citerai parmi les principaux de ces travaux :—

En Amérique, ceux de G. Bell et Tainter, qui ont créé, on peut le dire, cette nouvelle branche de la physique ;

En Angleterre, ceux de MM. Tyndall, Preece, Sylvanus Thompson, Moser, etc. ;

En Danemark, ceux de M. Röngten ;

En Suisse, ceux de M. Henri Dufour.

J'ajouterai à cette liste, si vous voulez me le permettre,

En France, les quelques travaux que j'ai pu effectuer moi même.

Depuis environ une année, il s'est accumulé un nombre de faits assez grand pour qu'on puisse essayer dès à présent, à ce qu'il me semble, d'en faire une classification au moins provisoire.

J'ai essayé d'indiquer cette classification sur ce tableau, et je vais en donner les bases. D'abord, la définition du mot lui même.

Classification des Phénomènes Radiophoniques.

Radiophonie	Directe	Thermique	... ...	Thermophones : air et gaz, vapeurs.
		Lumineuse	... ...	Photophones : peroxyde d'Azote, vapeurs d'Iode
		Actinique	... ...	... ...
	Indirecte	Thermique	Electrique ...	... ...
			... ...	... ...
		Lumineuse	Electrique ...	Photophones électriques : sélénium, alliages de sélénium et tellure : noir de fumée
			... ...	... ...
		Actinique	Electrique ...	... ...
			... ...	... ...

Si on veut définir scientifiquement et aussi brièvement que possible le mot " Radiophonie," on peut dire, que ce mot comprend les phénomènes relatifs à la transformation d'énergie radiante en énergie mécanique sous forme sonore. C'est je crois la définition la plus brève qu'on puisse en donner, mais il faut l'expliquer. À cet effet, si l'on considére ce qu'on appelle habituellement une source lumineuse, cette source, qu'on devrait appeler d'une manière plus générale une source radiante, est généralement très complexe : elle est formée ordinairement d'une infinité de radiations, dont les unes sont colorées et visibles pour l'œil, exerçant une certaine action sur la rétine, et dont les autres ne sont pas colorées, mais n'en existent pas moins.

(On projete un spectre sur un écran.)

Il y a (je rappelle ce fait bien connu) à gauche du spectre d'une source des radiations invisibles qui ont des propriétés toutes spéciales, il y en a à droite également. Chacune de ces radiations visibles ou invisibles (je le rappelle rapidement) posséde trois propriétés principales : elle est chaude, elle est lumineuse et elle peut produire des effets chimiques ou actiniques.

Chacune de ces radiations visibles ou invisibles est donée de ces propriétés ; mais il est certain que les effets thermiques sont concentrés dans les radiations qui avoisinent le rouge, et principalement dans les radiations invisibles au delà du rouge.

Tout le monde sait qu'un thermomètre promené le long de ce spectre donne un maximum dans le partie invisible, à peu près à l'endroit que j'indique ; d'autre part, les radiations visibles produisent leur effet sur la rétine, avec un maximum qui est dans les environs de l'orangé.

Les radiations violettes et ultra-violettes produisent principalement des effets chiminiques ou actiniques avec un maximum qui se trouve dans le spectre invisible ; de telle sorte qu'on peut adopter au moins provisoirement la classification suivante.

Si un effet quel qu'il soit, je ne le définis pas davantage pour le moment, est produit principalement par les radiations rouges et infrà-rouges avec un maximum à l'endroit que j'ai fixé tout à l'heure, on peut dire que cet effet est un effet *thermique;* si, au contraire, l'effet auquel je fais allusion est produit principalement par les radiations visibles avec un maximum aux environs de l'orangé et du jaune, on pourra dire que c'est un effet lumineux, ou ce qui vaudra mieux *photique.* Enfin, si le phénomène est produit principalement par les radiations violettes ou ultrà-violettes avec un maximum dans la région invisible, on pourra dire qu'on aura affaire à un effet chimique ou *actinique.* Au point de vue qui nous occupe, si on peut produire de la radiophonie à l'aide des rayons dits thermiques, on pourrait appeler provisoirement l'instrument ou appareil qui produirait l'effet radiophonique, un *thermophone.* Si l'appareil produit son effet dans les rayons lumineux, on pourra l'appeler *photophone.* Enfin, si cet appareil produit ses effets radiophoniques dans la partie actinique ou chimique, on pourra l'appeler *actinophone.*

Delà, la classification que voici. Mais cette classification ne comporte pas seulement les termes dont je viens de parler. En effet, d'après l'examen rapide que nous ferons des phénomènes, nous verrons que la transformation d'énergie radiante en énergie sonore qui constitue la radiophonie peut s'opérer de deux manières; ou bien *directement* sans intermédiaire d'où un premier genre de phénomènes et d'appareils qu'on peut appeler radiophones *directs.* Mais il y a aussi des appareils qui produisent la radiophonie *indirectement*, avec la nécessité d'intermédiaires. Ces intermédiaires, on peut concevoir qu'ils soient variables, mais actuellement on n'en connait qu'un, c'est l'énergie électrique.

On connait des appareils, des instruments qui transforment l'énergie radiante en énergie sonore par l'intermédiaire de l'énergie électrique, d'un courant électrique. Il est possible qu'on découvre plus tard d'autres agents intermédiaires; mais pour le moment on ne connait que celui-là. D'ailleurs cette radiophonie indirecte peut être thermique, photique ou actinique comme la radiophonie directe.

Si nous examinons la classification inscrite sur le tableau nous pouvons faire les remarques suivantes.

Dans la radiophonie directe, nous connaissons déjà des instruments produisant la radiophonie thermique, ou des *thermophones*, ce sont l'air et les gaz.

Nous connaissons des radiophones lumineux ou des *photophones* directs. MM. Bell et Tainter ont cité dans leurs mémoires le peroxyde d'azote et la vapeur d'iode (je reviendrai sur ce point tout à l'heure); mais nous ne connaissons pas encore de radiophones actiniques ou des *actinophones;* j'ai quelques raisons de croire qu'il peut en exister; j'ai fait quelques expériences à ce sujet; mais elles ne sont pas encore assez précises pour que je puisse rien affirmer.

Quant à la radiophonie indirecte, nous ne connaissons pas encore de radiophones thermiques indirects; mais il existe des radiophones lumineux, des photophones indirects, ce sont les photophones découverts par M. G. Bell, des photophones à sélénium, auxquels on a joint maintenant des photophones à alliages de sélénium et tellure, et, enfin, des photophones à noir de fumée: ce

sont les seuls qu'on connaisse actuellement. Quant à la radiophonie indirecte actinique, on n'en connait pas encore d'exemple. Vous voyez par conséquent que ce tableau présente d'assez grandes lacunes; il appartiendra aux physiciens qui voudront étudier ce sujet de le compléter, mais je ne puis parler, pour le moment, que des choses qui sont connues, et je laisserai même de côté les faits douteux en les indiquant simplement. Je prends donc pour base cette classification, et je vais indiquer d'abord les faits; ensuite, en quelques mots, autant du moins que je puis le connaitre, le mécanisme des transformations; et, en troisième lieu, si le temps le permet, quelques applications.

D'abord les faits. Je considére, en premier lieu, la radiophonie directe et en particulier la thermophonie directe.

Il faut divers appareils pour la réaliser; il faut prendre d'abord une source radiante, lumineuse si vous voulez, et il faut interrompre les radiations, les rendres intermittents.

Parmi les appareils interrupteurs qu'on peut employer, l'un des plus simples est celui que vous avez sous les yeux; c'est tout simplement une roue en verre, sur la quelle on a collé une bande de papier, dans laquelle on a decoupé avec un emporte-pièce, des ouvertures. On peut faire plusieurs séries d'ouvertures. Ici il y en a quatre, comprenant 40, 50, 60, et 80 ouvertures: c'est dans le but de pouvoir obtenir des sons formant un accord parfait majeur: on pourrait adopter tout autre mode de division, bien entendu. Cette roue qui est mise en mouvement par un moteur quelconque, soit à l'aide d'une manivelle et d'une corde, soit comme nous le faisons ici, dans le pavillon des postes et télégraphes, avec une turbine de M. Humblot, soit par tout autre moyen.

Lorsqu'on la met en mouvement, tantôt les radiations passent à travers les ouvertures, tantôt elles sont arrêtées par l'intervalle opaque qui les sépare, par conséquent en faisant tourner la roue plus ou moins vite, nous avons une série d'intermittences dont le nombre varie avec la vitesse. Si, par exemple, nous avons 80 ouvertures, et si nous donnons un tour par seconde à la roue, nous aurons 80 émissions de lumière et 80 interruptions.

Maintenant il s'agit de reçevoir ces radiations intermittents sur

un appareil qui permette de produire les sons. Or il est extrêmement simple de construire l'appareil qui peut réaliser un thermophone. Il y en a de plusieurs formes, je vais vous indiquer la plus simple, qui me parait consister dans un simple tube en verre, un tube à essais de chimie ordinaire, dans lequel on renferme un gaz. Ce gaz peut être l'air, l'acide carbonique, le protoxyde d'azote, peu importe. En mettant à l'extrémité de ce tube, un tuyau en caoutchouc et un cornet acoustique, il suffit de le placer derrière le faisceau lumineux intermittent, d'écouter, et on entend alors des sons dont la hauteur est en rapport avec le nombre des ouvertures qui passent dans un plan vertical par seconde.

Mais si on veut obtenir des effets suffisamment énergiques, un appareil ainsi construit donne des résultats assez faibles : en particulier, si on se contente de prendre un tube à air, il faut conserver le silence le plus profond, et employer des sources énergiques pour obtenir des effets sonores suffisants. Heureusement il est très facile de renforcer les sons ainsi produits par les gaz (je dis les gaz, je puis ajouter les vapeurs qui produisent les mêmes effets) : pour obtenir ce renforcement, il suffit de placer à l'intérieur de ce tube une substance susceptible d'absorber énergiquement les gaz et les radiations thermiques. Parmi ces substances, on peut citer le noir de fumée, l'encre de Chine, le bitume de Judée, le noir de platine et quelques autres. Pour ajouter, par exemple, ce noir de fumée, un des moyens les plus simples consiste à prendre une substance on peut dire quelconque, un morceau de clinquant de cuivre, un morceau de carton, du papier, n'importe quoi, pourvu qu'on puisse l'enfumer; et pour faire l'appareil (permettez moi de le construire sous vos yeux), il suffit de passer ce morceau de clinquant sur une flamme fumeuse, sur une flamme de lampe à huile, ou ce qui est encore plus simple, sur la flamme d'un morceau de camphre allumé; on enfume le clinquant des 2 côtés, on l'enfonce dans le tube, on y adapte un tube en caoutchouc, et l'appareil est fait.

Vous le voyez, cela n'offre aucune difficulté. Eh bien, si on compare les effets produits par l'air seul avec les effets produits par l'air qui se trouve ainsi en contact avec le noir de fumée, il y a une différence d'intensité considérable que je ne puis pas évaluer exactement, mais qui est au moins 20 fois plus grande.

Maintenant, il s'agit de démontrer que cet appareil est un *thermophone* conformément à mon affirmation. Pour le démontrer, l'expérience est assez simple, il suffit de faire passer à travers la roue radiophonique une radiation, et puis, avec un prisme à la manière ordinaire, de produire le spectre de la radiation et de l'étaler.

Bien entendu, il faut limiter la lumière par une fente, ce qui, malheureusement, fait perdre beaucoup de lumière et diminue beaucoup l'intensité du faisceau ; il en résulte que ces expériences là ne peuvent être faites que dans un laboratoire très calme, et dans l'obscurité, autant que possible. Il suffit alors de promener le tube le long du spectre, en faisant mouvoir la roue. Mais pour que l'expérience puisse être faite avec beaucoup de précision, on peut remarquer qu'il n'est pas nécessaire de prendre des tubes de grand diamêtre, c'est absolument inutile ; un simple tube comme celui que je montre ici, lequel a à peu près deux millimètres de diamètre, suffit parfaitement. Il y a là dedans un petit morceau de clinquant enfumé, et cet appareil peut être considéré comme tout à fait analogue à un élément de pile thermo-électrique linéaire; car on peut le promener le long du spectre, de façon à n'occuper que 0·002m. Eh bien, si on fait cette expérience, de façon à avoir le cornet acoustique à l'oreille, voici ce que l'on constate. Dans la partie ultra-violette on n'entend absolument rien ; il en est de même pour le violet, le bleu, le vert, et le jaune (au moins, je fais cette restriction, avec le soleil de Paris, nous allons voir tout à l'heure ce qui se passe avec le soleil de Washington). Avec le soleil de Paris, ou avec la source éléctrique lumineuse la plus intense dont j'ai pu me servir, on ne commence à entendre un effet qu'à la limite de l'orangé et du jaune, c'est-à-dire, à peu près à l'endroit où commencent précisément les effets thermiques du spectre. L'intensité sonore augmente au fur et à mesure qu'on arrive dans le rouge ; dans le rouge invisible, elle augmente encore, et l'on arrive à un maximum. Or, si on note l'endroit où se produit ce maximum, ou trouve qu'il coincide avec l'endroit où l'on obtient le maximum d'effet avec un pile thermo-électrique. Si on continue de faire mouvoir l'instrument, l'effet décroit et il décroit précisément comme l'effet thermique.

On peut donc déjà conclure, messieurs, de cette expérience, qu'il est infiniment probable qu'on à là un appareil thermophonique; mais on peut aller plus loin, on peut faire une expérience extrêmement simple qui démontre le fait, je crois, jusqu'à l'évidence. Cette expérience consiste à produire avec ces appareils, un effet véritablement intense avec des radiations chaudes tout à fait invisibles.

A cet effet, on prend, par exemple, un disque de cuivre qu'on peut placer devant la roue tournante, pour remplacer la source lumineuse; on chauffe cette plaque de cuivre, par exemple, avec un bec de Bunsen ou avec un chalumeau oxhydrique, jusqu'à ce qu'elle commence à être rouge dans l'obscurité. Alors, on entend des sons très intenses; on éteint la source calorifique, la plaque se refroidit, et elle finit par devenir presque immédiatement invisible dans l'obscurité. Eh bien, on continue à entendre les sons avec la même intensité. Par conséquent, une plaque chauffée à 300°, qui est parfaitement invisible dans l'obscurité la plus complète, donne des radiations que produisent sur les appareils que je viens d'indiquer des sons intenses.

Je crois donc que ces expériences suffisent pour prouver qu'on a bien affaire dans un instrument de ce genre à un véritable *thermophone*.

Maintenant, messieurs, je passe des thermophones directs aux photophones. MM. Bell et Tainter, dans le belles études qu'ils ont faites sur la série des gaz et des vapeurs qui produisent les effets radiophoniques ont cité dans leurs mémoires deux gaz qui, soumis à l'expérience dont je viens de parler, avec un appareil analogue à celui-là, ne sont par sensibles aux mêmes radiations. Par exemple, le peroxyde d'azote donne des effets sonores dans les radiations *lumineuses* avec un maximum dans le bleu, et la vapeur d'iode donne des effets sonores dans les radiations visibles, avec un maximum dans le vert. MM. Bell et Tainter ont cité aussi quelques autres gaz, mais, cependant, en émettant des doutes, de sorte que pour le moment, en fait de photophones directs, on n'est parfaitement certain que de ces deux gaz: le peroxyde d'azote et la vapeur d'iode, mais il est possible, il est probable même qu'on en trouvera d'autres.

Enfin, comme je l'ai déjà fait remarquer il n'y a pas d'actinophones directs pour le moment.

Avant de passer à la radiophonie indirecte, je dois faire remarquer que le tableau des thermophones, photophones, et actinophones directs ne comprend que des gaz ou des vapeurs; il n'y a ni corps liquides, ni corps solides, je n'ai pas cru devoir mettre les corps liquides dans ce tableau et voici pourquoi.

Lorsqu'on veut faire l'experience sur les corps liquides (projection d'un tube), on peut s'y prendre de la manière suivante; on peut prendre comme l'indique cette projection, un tube comme celui-ci, muni de son tube en caoutchouc et du cornet acoustique; on peut verser à la partie inférieure un liquide, de sorte qu'en chauffant légèrement si c'est nécessaire, on a à la partie inférieure une couche liquide; au dessus, la vapeur de ce liquide mélangée plus ou moins avec de l'air; puis on peut, dans la partie supérieure, mettre sur le bord du tube une morceau de clinquant enfumé.

On peut exposer la partie inférieure aux radiations du soleil ou d'une source électrique; puis, en faisant glisser le tube, on peut y exposer la vapeur et puis enfin la vapeur avec une couche de noir de fumée derrière elle.

Eh bien, si on emploie la lumière électrique, on ne parvient pas à obtenir des effets sensibles dans les liquides, du moins M. Tyndall n'a jamais pu en obtenir, moi même j'ai essayé avec du soleil, mais, je le répéte avec le soleil de Paris, je n'ai jamais pu obtenir aucun effet dans aucun liquide.

MM. Bell et Tainter dans leurs mémoires, citent un certain nombre de liquides, mais avec la plupart d'entre eux ils n'ont pu obtenir d'effets: cependant, ils citent en particulier l'éther sulfurique comme un liquide donnant (sous l'influence des radiations du soleil de Washington, c'est-à-dire, dans une radiation extrêmement énergique dont nous ne jouissons jamais ici malheureusement), des effets excessivement faibles, mais distincts. Dès-lors, messieurs, du moment qu'il est absolument nécessaire d'avoir une radiation aussi énergique que celle que citent MM. Bell et Tainter et qu'on n'atteint avec cette radiation que des effets extrêmement faibles, il nous semble qu'il reste certains doutes sur les résultats. Remarquez, d'ailleurs, que lorsqu'on soumet un liquide comme

l'éther sulfurique aux radiations très intenses du soleil, il est certain qu'une portion de l'éther, lequel bout vers 35° se vaporise de sorte qu'il peut y avoir entre le tube et le liquide une couche de gaz, et il serait fort possible que ce fût cette couche de gaz qui donnât les sous extrêmement faibles qu'on obtient dans ce cas là. Quoiqu'il en soit la question m'a paru assez douteuse pour que je ne croie pas qu'on puisse,—à moins d'expériences plus concluantes et plus nouvelles,—placer les liquides dans cette catégorie.

Quant aux corps solides, il est certain que lorsqu'on expose un corps solide à des radiations, on obtient des effets sonores. Mais quelle est le cause de ces effets? Ici il s'est élevé une discussion entre M. Preece et M. Bell. M. Bell affirmait que lorsqu'un corps solide est soumis aux radiations, il vibre par lui même. M. Preece qui, je crois, est encore dans le même ordre d'idées, pensait que ces effets étaient dûs à la couche de gaz qui est en contact avec le corps solide, ainsi que je l'avais indiqué dès le mois de Décembre, 1880.

Je ne puis pas, malheureusement, le temps dont je dispose ne me le permet pas, donner des détails sur cette discussion très intéressante, mais comme j'ai fait des expériences nombreuses à ce sujet, je crois que lorsqu'un corps solide est soumis à une radiation intermittente, si ce corps vibre par lui même conformément à une théorie qui a été émise par lord Raleigh, la vibration est infiniment petite par rapport à celle qui est due aux gaz en contact avec le corps; de telle sorte que dans l'effet sonore produit, il peut y avoir une portion qui est due au corps solide, mais cette portion est infiniment petite eu égard à l'autre.

Donc, dans l'état, la question reste au moins douteuse, et c'est pour cela que je n'ai pas mis les corps solides dans cette classification, qui du reste, comme je l'ai déjà dit, est absolument provisoire.

Cela posé, je passe aux instruments produisant la radiophonie indirecte. Comme vous le voyez, nous n'avons pas encore des radiophones indirects thermiques, nous n'en connaissons pas; mais nous connaissons des radiophones indirects lumineux, des *photophones* produisant la transformation d'énergie radiante en énergie sonore par l'intermédiaire d'un courant électrique. Pour produire cet effet, le premier qui ait été découvert par MM. G. Bell et Tainter, l'appareil est extrêmement simple: il suffit de prendre

comme transmetteur la roue ordinaire, la roue radiophonique, et comme recepteur plusieurs corps, et d'abord le sélenium.

La sensibilité du sélénium sous l'influence des rayons lumineux a été découverte par M. Willoughby Smith, étudiée par lui et puis, par plusieurs autres observateurs, MM. le professeur Adams, Sale, lord Rosse, W. Siemens qui a jeté beaucoup de lumière sur cette question, et, enfin, dans ces derniers temps, par M. Bell qui a trouvé moyen de constater la sensibilité du sélénium sous l'influence de l'énergie radiante (je laisse le terme vague) en rendant les radiations intermittentes, et en substituant à l'emploi du galvanomètre comme moyen d'étude l'emploi du téléphone.

Si l'on prend un récepteur à sélénium que je vais décrire tout à l'heure, et si on le place derrière une radiation intermittente produite par cette roue; si on met cette plaque de sélénium dans le circuit d'une pile et qu'on intercale dans ce circuit un téléphone, toutes les fois que les radiations lumineuses tombent sur le sélénium il se produit un effet sur le mécanisme duquel je reviendrai tout à l'heure si le temps me le permet,—qui n'est pas encore très connue,—mais, enfin, en vertu duquel les choses se passent comme si l'intensité du courant variait. Il en résulte dans le téléphone des variations correspondantes, et le téléphone reproduit les sons comme le faisait tout à l'heure le thermophone. Ainsi, une radiation, une roue tournante, un récepteur en sélénium que je vais indiquer, un courant électrique dans lequel se trouve un pile et un téléphone, voilà l'appareil qui constitue le photophone à sélénium.

Il y a, messieurs, plusieurs formes de ces récepteurs: celle qu'a décrite M. Bell est un peu compliquée. À l'école supérieure de télégraphie, nous employons pour ces études des récepteurs qui sont plus simples que ceux qui ont été indiqués d'abord, plus faciles à construire, et surtout très faciles à réparer lorsqu'ils viennent à se détériorer. On va projeter sur le tableau l'image d'un de ces récepteurs.

(On projete le dessin d'un récepteur.)

Comme on le voit, ce récepteur se compose de deux tiges en cuivre reliées l'une à l'autre par deux vis isolées par un cylindre d'ivoire et d'un bloc de métal et de papier.

Pour le former, on prend deux lames de clinquant de matières diverses qu'on enroule en les séparant par du papier parchemin qui les isole, de telle manière que les lames ne communiquent qu'avec les tiges extérieures.

Lorsqu'on a enroulé ainsi cet appareil sur lui même, ou le met dans un étau, on lime avec beaucoup de soin les 2 surfaces, en s'assurant qu'il n'y a pas de communication entre les spires produite par des grains de limaille. On effectue ensuite le séléniage. À cet effet, on place le récepteur sur une plaque de cuivre, on la chauffe soit avec un lampe à alcool soit avec un bec Bunsen; puis, on prend un crayon de sélénium sous la forme où on le trouve dans le commerce, c'est-à-dire, sous la forme de cylindre et on l'appuie sur l'appareil. Lorsqu'on a atteint la température d'environ 217° le sélenium commence à fondre: à partir de ce moment là il suffit de le promener 8 ou 10 fois à la surface de l'appareil, d'éteindre la flamme, et de laisser refroidir. L'appareil se trouve ainsi terminé, l'une des surfaces se trouve recouverte d'un couche de sélénium excessivement mince, et cette opération très simple suffit pour constituer le photophone. Maintenant, si on veut conserver cette couche et la mettre à l'abri des accidents, ou peut la vernir à la gomme laque de façon à conserver l'isolement de la seconde face et de conserver intacte la surface en sélénium.

Je dois dire que quant à la nature du métal qui constitue comme les armatures de cette sorte de condensateur, beaucoup de métaux donnent de très bons effets. Je citerai le laiton, le cuivre rouge, le fer et le platine qui donnent des effets excellents; avec l'argent, il ne m'a pas été possible de constituer une récepteur à sélénium, et je crois que cela tient à ce que l'argent attaque énergiquement le sélénium et le séléniure d'argent produit est trop bon conducteur de l'électricité. Avec l'aluminium qui serait très commode pour construire ce récepteur, on ne peut pas davantage avoir d'effets, mais ici c'est par une raison inverse, cela tient à ce que sélénium n'adhére pas du tout à la surface, sur laquelle il se dépose en gouttes huileuses: mais enfin nous employons couramment la platine, le laiton, le cuivre rouge. Je ferai une remarque sur ces appareils. Suivant la manière dont on les sélénie, suivant l'épaisseur de la couche qu'on met sur l'appareil on peut obtenir

(et cela est très important pour les applications) des résistances électriques de la couche de sélénium extrêmement variables; on peut obtenir des photophones de ce genre dont la résistance peut varier de 3,000 à 300,000 ohms, et qui, néanmoins, avec une pile très faible de 3, 4, ou 5 élements Leclanché donnent de bons résultats, c'est une propriété qui pourra servir de base à des applications qui me paraîssent très importantes.

À présent, messieurs, il s'agit de démontrer que cet instrument ainsi construit est bien un *photophone* et non pas un *thermophone*. Eh bien, pour le démontrer, on peut faire avec cet appareil la même expérience que celle que j'ai indiquée à l'aide du petit élément thermophonique.

En effet, il en facile de remarquer—et ceci est encore très important pour les applications—qu'il n'est pas nécessaire d'illuminer la surface entière d'un élément de ce genre pour produire l'effet sonore; il suffit d'en illuminer un ou deux millimètres carrés. Par conséquent, on peut avec du papier, avec un corps opaque recouvrir presque complètement la surface, de façon à ne laisser libre qu'une bande étroite de sélénium. On peut promener eusuite cette baude de sélénium le long du spectre pourvoir quelles sont les radiations qui agissent. Lorsqu'on fait cette expérience voici ce que l'on constate,—au moins, je fais toujours cette réserve, avec le soleil de Paris et avec la lumiére électrique telle qu'on peut l'obtenir ici,—on constate que dans l'ultrà-violet et le violet on n'obtient pas de sons perceptibles: le son commence à la limite du violet et de l'indigo; dans le bleu, il augmente d'intensité, et peu à peu on arrive à un maximum dans le jaune orangé. Puis, à partir de l'orangé, le son décroit et termine avec le rouge. Je n'ai jamais pu obtenir, jusqu'ici, de sons perceptibles au delà de la partie visible. Il en résulterait alors que l'instrument est un photophone, puisque les seules radiations qui agissent sur lui sont des radiations lumineuses.

M. Bell a repris ces expériences et il a constaté une légère différence entre les résultats qu'il a obtenus et ceux que j'indique ici, mais la différence est extrêmement faible. M. Bell n'a fait l'expérience qu'avec le soleil intense de Washington qui est a peu près analogue à celui d'Algérie ou du Maroc. Eh bien, avec ce

soleil, M. Bell a trouvé également que l'instrument n'était sensible que dans les radiations lumineuses; seulement, comme la source avec lequelle il opére est plus intense que la mienne, il a constaté que le maximum se déplace légèrement vers le rouge mais il n'a pas obtenu de son dans le rouge invisible, il a constaté en outre, que les radiations actives s'étendaient un peu dans le violet. Il n'en résulte pas moins que les deux genres d'expériences sont concordantes pour montrer que les radiations actives, ici, sont les radiations lumineuses. Donc, on a affaire ici à un veritable *photophone.*

Maintenant, au lieu de prendre du sélénium, on peut prendre des alliages de sélénium et de tellure.

Je n'ai par expérimenté ces alliages, je ne peux donc pas en parler, mais on peut employer un corps très singulier; le noir de fumée, dont je parlais tout à l'heure comme substances renforçante dans le thermophone.

Lorsqu'on constitue un récepteur photophonique au noir de fumée, lorsqu'on fait passer un courant électrique à travers le noir de fumée, il est sensible à l'action des radiations.

Je vais projeter le récepteur au noir de fumée qui a été imaginé par M. Bell (c'est le récepteur primitif), il se compose

(On projete l'image de ce recepteur.)

tout simplement d'une lame de verre argentée, sur laquelle on a enlevé, avec la machine à diviser l'argent, de façon à former des zigzags: il en résulte que la plaque se trouve divisée en deux régions, la région de droite séparée complètement de la région de gauche.

Cela fait, on enfume cet appareil, on le recouvre de noir de fumée, et alors le courant électrique ne peut passer d'une région à l'autre qu'on traversant le noir de fumée. Mais ces récepteurs sont assez fragiles. Quand on a à sa disposition un récepteur photophonique comme celui que je viens de décrire, rien n'est plus simple que d'en faire un récepteur à noir de fumée. Il suffit de le promener, tout simplement, comme je le faisais tout à l'heure pour le thermophone, sur une flamme fumeuse, de telle sorte qu'on peut avoir, avec cet appareil-là, d'un côté une face séléniée, et, de l'autre côté, une plaque enfumée; et alors en les exposant aux

radiations tantôt sur une face et tantôt sur l'autre, on peut comparer ainsi très facilement les effets du noir de fumée avec les effets du sélénium. Eh bien, on constate par ce moyen, que les effets du noir de fumée sont tout à fait analogues à ceux du sélénium; seulement ils sont beaucoup moins intenses, de telle sorte que si on a intérêt, pour des applications, à se servir de récepteurs intenses, il faut employer le sélénium. Il n'y a pas de doute à cet égard.

Maintenant, messieurs, je passe à la seconde partie de ma tâche, et je dirai un mot très court sur le mécanisme des transformations qui se produisent, lorsqu'une radiation intermittente tombe sur l'un des récepteurs décrits.

Commençons par les thermophones. Que se produit-il quand ils vibrent? Il me paraît évident que, du moment que l'instrument n'est sensible qu'aux radiations thermiques, il est naturel d'admettre que le gaz ou la vapeur se trouve échauffé lorsque les radiations passent à travers les ouvertures de la roue interruptrice; qu'il se refroidit lorsque les radiations sont éteintes, de telle sorte qu'on a des alternatives très rapides d'échauffement et de refroidissement, par conséquent production de vibrations de la façon bien comme en acoustique.

Dans cette manière de voir, le noir de fumée et les substances de ce genre agiraient en condensant une couche de gaz et en absorbant énergiquement les radiations thermiques de façon à augmenter seulement l'effet produit. Cette manière d'expliquer le phénomène, si elle est vraie, donne comme conséquence qu'un tube de cette nature ne doit pas différer, au fond, d'un tuyau sonore ordinaire. C'est en effet ce qui a lieu, et je vais revenir sur ce point tout à l'heure. Mais auparavant, je désire parler (je serai très bref et pour cause) du mécanisme de la transformation qui se produit dans le cas du photophone à sélénium ordinaire. Ici plusieurs opinions sont en présence: celle de M. le professeur Adams qui admet qu'il se produit là une force électro-motrice particulière; celle de M. Siemens qui admet qu'il se produit une variation de conductibilité dans la plaque de sélénium, et, enfin, une troisième opinion qui a été émise récemmant par M. Moser, en vertu de laquelle la plaque agit par son contact imparfait avec les

spires métalliques, de telle sorte que l'appareil constituerait une sorte de microphone lorsqu'il serait frappé par les radiations intermittentes. Je n'ai pas pu encore étudier suffisamment cette question, par conséquent je ne puis pas donner mon opinion; j'ignore encore quelle est celle de ces trois opinions qui est la vraie, ou bien si elles sont vraies toutes les trois, ce qui pourrait bien être.

Enfin, messieurs, pour terminer ce discours un peu trop long, je voudrais indiquer très rapidement des applications que peuvent avoir les appareils de ce genre. Nous sommes aujourd'hui à une époque où, lorsqu'on présente un appareil, un instrument, on éprouve le besoin de dire : à quoi cela peut il servir ? et, dans les expériences que nous faisons, d'après les ordres de M. le Ministre des postes et télégraphes, dans le pavillon réservé au Ministère dans l'Exposition, chaque fois que nous montrons au public des phénomènes de ce genre, sur cinquante personnes, il y en a quarante-huit qui disent : à quoi cela peut-il servir ? Je sais bien que des questions de cette nature ne sont que d'ordre secondaire pour une assemblée comme celle-ci. Cependant il n'en est pas moins vrai que, si on peut trouver des applications pratiques, industrielles, de phénomènes qui, jusqu'ici, paraîssent renfermés dans le domaine scientifique, c'est un avantage qu'on est en droit de rechercher. Eh bien, je crois qu'il y a, d'ores et déjà, plusieurs applications possibles de ces phénomènes, et des applications de divers genres.

Permettez moi d'abord de vous signaler une application d'ordre scientifique, mais qui, cependant, pourrait avoir une valeur pratique : et peut-être même à propos des travaux qu'effectuera la commission internationale des unités électriques, car elle sera probablement amenée à déterminer l'équivalent mécanique de la chaleur. Eh bien, reprenons un tube thermophonique. Je viens de dire tout à l'heure que ce tube ne devait pas différer d'un tuyau sonore : il ne doit y avoir entre eux qu'une seule différence : au lieu de percer une embouchure, et l'insuffler dans le tube thermophonique un courant de gaz ou un courant d'air pour le faire résonner, on l'expose simplement aux effets d'une radiation lumineuse. Il doit donc se produire dans le thermophone

des vibrations analogues à celles qui se produisent dans un tuyau d'orgue ordinaire. Effectivement, avec un thermophone, on peut affirmer qu'il est possible de reproduire toutes les expériences que l'on fait d'habitude sur les tuyaus sonores; je vous citerai en particulier les expériences de Dulong. On peut disposer un long appareil de ce genre, de façon à le fermer à un bout par une petite lame de mica, à lui adapter, par exemple, un cornet acoustique pour pouvoir entendre les sons qui sont produits à l'intérieur. Quand on l'attaque avec une radiation, on peut entendre les sons.

Si alors on y enfonce un piston muni d'une longue tige, de façon à pouvoir le promener le long du tube, quand on produit les sons, et qu'on enfonce le piston dans le tube, il arrive un moment où les sons cessent complètement; si on continue à enfoncer le piston, les sons recommencent en augmentant d'intensité; on arrive à un maximum et puis on trouve un second point où le son disparaît. On a donc ainsi deux nœuds et un ventre de vibration, comme dans les tuyaux sonores, et ils sont identiques, car si on mesure la distance entre les 2 nœuds, on retrouve la demi-longueur d'onde relative à la propagation du son dans l'air. Il en résulte qu'on peut déterminer la vitesse du son à l'aide de ce tube, et comme il n'y a pas ici d'embouchure, et par suite les perturbations qui en résultent, on est en droit de penser que les expériences de Dulong pour déterminer la vitesse du son pourront se faire dans un tuyau thermophonique avec plus d'exactitude qu'avec les tuyaux ordinaires. On peut d'ailleurs enfermer dans ce tube des gaz quelconques, on peut les chauffer à une température bien spécifiée et, par conséquent, essayer de déterminer ainsi la vitesse du son dans des gaz ou des vapeurs à pression et à température variables, et comme l'équivalent mécanique de la chaleur est lié à cette question, il est possible (je pose ici un point d'interrogation)—il est possible qu'un appareil de ce genre puisse permettre d'effectuer cette détermination. Ceci constituerait une application d'ordre scientifique.

En voici une seconde d'un autre ordre. Lorsqu'on examine ce qui se passe avec des roues interruptrices dont on peut faire varier la vitesse, et si l'on cherche à voir les effets sonores de

diverses natures qu'on peut obtenir avec un appareil de ce genre, on constate ceci : d'abord en faisant croître la vitesse de la roue, on peut obtenir, soit avec le thermophone, soit avec le photophone, des sons musicaux dont le nombre de vibrations peut varier depuis les plus graves que l'oreille puisse entendre, jusqu'à des sons dont le nombre des vibrations peut aller jusqu'à 4,800 par seconde, et au delà. Non-seulement on peut obtenir des sons uniques, mais encore des accords. Ainsi, avec la roue qui est ici, en concentrant les rayons avec une lentille cylindrique, on peut utiliser simultanément les quatre séries d'ouvertures qui sont là et obtenir alors dans le photophone ou le thermophone, des accords parfaits. Mais on peut aller plus loin ; on peut, avec un appareil de ce genre, ou avec un appareil analogue à celui-là, reproduire non-seulment des sons et des accords musicaux, mais encore le chant et la parole articulée. Pour reproduire le chant (je vais en dire un mot rapidement et rappeler le phénomène) pour reproduire le chant et la parole articulée, on peut employer un appareil de ce genre, appareil qui' a été, du reste, je m'empresse de le dire, imaginé à peu près sous cette forme-là par M. Bell. On peut faire tomber les radiations sur une lame de verre argentée, que voici, extrêmement mince, qui a à peu près 1/10 ou 1/12 de millimêtre d'épaisseur. Le rayon lumineux tombant sur cette lame est réfléchi, il est concentré à l'aide d'une lentille, et on place soit au foyer soit plutôt dans le plan où se produit l'image de la plaque, un thermophone ou un photophone.

Voici un moyen de faire cette expérience dans un laboratoire assez facilement. Je suppose qu'on puisse avoir à sa disposition un cabinet comme l'indique ce dessin. (On projete un dispositif d'expérience.) On prend en main un cornet acoustique fixé à un long tube de caoutchouc qui peut avoir 10, 12, 18 mètres de longueur : le cabinet étant fermé, on peut chanter et parler dans le cornet, en étouffant les sons si c'est nécessaire, à l'aide d'un système analogue à celui qu'on emploie à l'exposition pour faire entendre des auditions téléphoniques. Les vibrations du chant ou de la voix viennent se transmettre à l'appareil muni de la lame de

verre argenté extrêmement mince placée dans une autre chambre, et sur laquelle on fait tomber un faisceau de rayons parallèles provenant d'une source de lumière électrique, ou bien d'un rayon solaire au moyen d'un héliostat.

Le faisceau réfléchi n'est pas parallèle parce que jamais des lames aussi minces ne sont planes, elles sont toujours plus ou moins courbes. Maintenant si on dispose d'une seconde chambre séparée de la première par une glace sans tain on peut faire passer le faisceau lumineux réfléchi à travers cette glace, et le concentrer soit à l'aide d'un miroir concave, soit à l'aide d'une lentille sur un thermophone ou un photophone. De cette manière un observateur peut chanter et parler dans le cornet acoustique, il n'est pas entendu par l'opérateur qui écoute.

Les vibrations de la voix produisent des déformations de la lame argentée et des variations correspondantes d'intensité dans le faisceau lumineux réfléchi, et l'on peut entendre, dans ces conditions le chant ou la parole articulée. Entendons-nous ; pour le chant, il n'y a aucun doute, on entend parfaitement le chant avec le soleil, avec la lumière électrique et même avec la lumière oxhydrique (je n'ai pas pu encore descendre au dessous). Mais quant à la parole articulée, la difficulté est beaucoup plus grande. Néanmoins, lorsqu'on peut se servir d'un rayon solaire suffisamment intense, on obtient avec une netteté parfaite, surtout avec un thermophone, la reproduction de la parole articulée, et cette reproduction a cela de remarquable que le timbre n'est pour ainsi dire pas altéré, tandis que, dans les récepteurs photophoniques à sélénium, dans lesquels on emploie comme intermédiaire le téléphone, il y a toujours une altération.

Eh bien, messieurs, puisqu'on peut ainsi reproduire avec les plus grande facilité des effets musicaux, on peut songer à faire des instruments de musique pratiques avec des appareils de ce genre. Il y a ici un modèle d'appareil qui constitue un véritable piano, un piano que je pourrais appeler radiophonique. En effet, il y a ici dans la roue interruptrice quatre séries d'ouvertures formant un accord parfait. Il y a de plus (comme pourront le voir de près les personnes qui voudront, après la séance, examiner

l'appareil) il y a des tiges qui ferment ces ouvertures. Ces tiges sont mises en relation par un renvoi de mouvement très simple avec des touches de piano, de façon à ce que, lorsqu'on abaisse ce système de touches, on laisse la lumière passer. Alors, en mettant la roue en mouvement, et en appuyant sur les touches, on peut reproduire ou bien les sons de l'accord parfait, ou bien des sons successifs de cet accord. Je fais construire par M. A. Duboscq une roue de ce genre qui aura 21 séries d'ouvertures de façon à former une octave et demie avec tons et demi-tons. Nous aurons 21 tiges correspondant à 21 touches de piano, et nous pourrons, en faisant tourner la roue et en lui donnant une vitesse constante, jouer avec cet instrument, avec le même mécanisme dont on se sert pour le piano: et, si la personne qui joue fixe à son oreille le tuyau d'un long thermophone, placé derrière la roue tournante éclairée, elle pourra jouer du piano, entendre ce qu'elle fait; et, bienfait inestimable pour les voisins! les voisins n'entendront absolument rien. On aura donc constitué un piano silencieux, et ce sera un véritable service que la radiophonie pourra rendre à l'humanité.

Il y a d'autres applications. Malheureusement le temps me presse; je ne puis pas, comme je l'aurais voulu, vous indiquer une autre application susceptible, à ce que je crois, de devenir très importante, une application télégraphique. Je suis obligé de réserver ce sujet qu'il serait trop long de traiter en ce moment; mais je puis encore signaler une autre application des propriétés du sélénium, qui, je crois, sera indiquée par M. Bidwell dans une prochaine séance de la société, c'est la téléphotographie; application, qui, si elle aboutit, sera très remarquable.

En résumé, vous le voyez, messieurs, nous avons là des effets vraiment extraordinaires, dont la connaissance est dûe aux efforts de beaucoup de travailleurs, et qui constituent déjà une nouvelle branche de la physiqne. Indépendamment de leur valeur théorique, il me semble que j'ai pu vous faire entrevoir que ces phénomènes peuvent être susceptibles d'applications pratiques.

Il est à remarquer d'ailleurs qu'ils ont été trouvés presque simultanément par des physiciens qui ont étudié la question au

même moment, et indépendamment les uns des autres, dans toutes les régions du monde. C'est un exemple curieux du genre de travail qui sera peut être le seul possible dorénavant, avec la rapidité prodigieuse des communications: c'est aussi un exemple de la confraternité scientifique qui peut s'établir entre les savants de différent pays. J'ai déjà indiqué plusieurs noms au début de cette conférence, mais je ne jaurais la terminer, sans rappeler que si les phénomènes radiophoniques conduisent à des applications, et à des applications utiles, il faut en reporter en grande partie l'honneur à ceux qui en ont fait la découverte, à MM. G. Bell et Tainter.

A vote of thanks having been given to M. Mercadier, the President announced that the Special Meeting was adjourned until Saturday, the 24th September, at 8.30 p.m., and that all present were cordially invited to attend.

The Extraordinary General Meeting, adjourned from Thursday, September 22nd, was held on Saturday, September 24th, at 8.30 p.m., in the Electrical Exhibition, Palais de l'Industrie, Paris—Professor G. C. FOSTER, F.R.S., President, in the Chair.

After the Minutes of the last Meeting had been read and approved,

The PRESIDENT called upon Mr. Shelford Bidwell for his paper.

TELEGRAPHIC PHOTOGRAPHY.

By SHELFORD BIDWELL, M.A. (Associate).

Mr. SHELFORD BIDWELL: I cannot begin this paper without first expressing my extreme regret that I am insufficiently acquainted with French to be able to render it in that language. I will therefore ask the Honorary Secretary (Mr. Aylmer) to give a short explanation in French of what I am about to say.

M. AYLMER: M. Bidwell me prie de vous dire que l'appareil qu'il a pour but de vous décrire n'est que d'une nature tout à fait expérimentale. Son objet est de démontrer la possibilité de transmettre l'image des objets naturels à une distance voulue, au moyen du télégraphe, et d'indiquer une méthode par laquelle ce procédé peut être mis en pratique. Les images qui ont été jusqu'à présent transmises sont d'un caractère rudimentaire, se composant principalement de dessins simples en noir et en blanc, lesquels ont été produits par la lanterne magique. Mais il n'y a pas de doute que ce procédé est capable de très grandes améliorations, surtout quant à la nature des substances chimiques employés pour sensibiliser le papier: par l'adoption d'un systeme de développement analogue à celui dont on se sert dans la photographie ordinaire, l'appareil peut être rendu capable de télégraphier à une distance de plusieurs centaines de kilomètres des reproductions assez parfaites de paysages, et voire même des portraits. Mais il faut avoir fait beaucoup d'expériences avant de pouvoir atteindre une pareille perfection.

Mr. SHELFORD BIDWELL, proceeding: It may be well to state

that the apparatus which I am about to describe is merely of an experimental nature. My object is to demonstrate the possibility of transmitting pictures of natural objects to a distance by means of electricity, or by telegraph, and to indicate a method by which such a result may be practically accomplished. Hitherto the pictures actually transmitted by this process have been of a very rudimentary character, consisting mostly of simple designs in black and white projected upon the transmitter by a magic-lantern. But there can be no doubt that the process is capable of very great improvement, especially in regard to the nature of the chemicals employed to sensitise the paper used.

The invention consists in the application of two natural facts: first, that a colourless chemical compound can be decomposed by a current of electricity into two constituents, one of which possesses colour; and secondly (a fact discovered by Mr. Willoughby Smith), that the resistance offered by a piece of crystalline selenium to the passage through it of a current of electricity depends upon the amount of light to which the selenium is exposed, this resistance being greater in the dark than in the light.

The first of these facts was applied forty years ago by Bain in his electro-chemical telegraph, and subsequently by Bakewell in his copying telegraph, several modifications of which are now in the Electrical Exhibition.

If a piece of paper which has been soaked in a solution of iodide of potassium be laid upon a metal plate which is connected to the negative pole of a battery, and if a platinum wire which is in connection with the positive pole of the battery be drawn over the paper, the path of the point will be indicated by a brown line due to the liberation of iodine. The intensity of this brown line will vary with the strength of the current, and will cease altogether if the current is interrupted. If a series of closely parallel lines of such a kind are marked upon the paper, the appearance it will present on its surface will be one of a uniformly brown colour.

Such a coloured surface is the groundwork of Bakewell's pictures, and the designs upon it are produced simply by introducing breaks in the lines at the proper places.

In the Bakewell transmitter a platinum point is made to move

in a series of closely parallel lines across a picture drawn with a nonconducting resinous ink upon a sheet of tin-foil. The positive pole of a battery is connected to this point, the tin-foil to the line wire, and the distant end of the line wire to a second point which moves synchronously with the first across a piece of sensitised paper in a series of lines corresponding to those described upon the tin-foil. When the first point is in contact with the bare tin-foil, the current passes along the line to the second point, which consequently traces a brown mark upon the paper. But when the first point is upon a spot which is covered by the nonconducting ink, the current is interrupted, and no mark is made upon the paper by the second point. Thus it is evident that when the whole surface of the prepared tin-foil has been traversed by the first point there will appear upon the sensitised paper a design in white upon a dark ground produced merely by breaks in the parallel lines traced by the second point, and corresponding exactly with the orignal drawing upon the tin-foil.

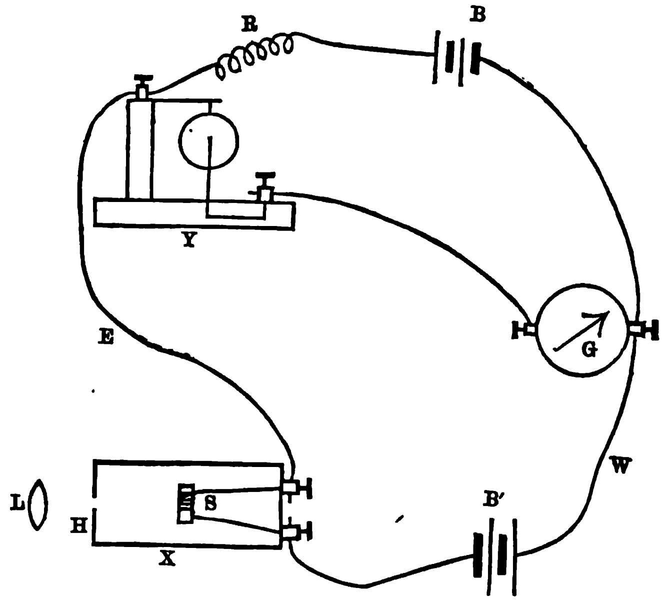
In my apparatus the receiving instrument is similar in principle to that of Bakewell, but the method of transmission is entirely different, inasmuch as the pictures intended to be transmitted are not mere artificial drawings, but the projected images of the actual objects themselves, which are focussed as in a photographic camera, upon a white screen contained in the instrument.

The method in which this is effected depends upon the application of the second of the natural facts previously mentioned, viz., that the electrical resistance of selenium is varied by the action of light.

The general arrangement of the apparatus is shown in the diagram on the next page.

The receiving instrument in this apparatus contains a brass cylinder, covered with platinum foil, which is about two inches long and seven-eighths inch in diameter. It is a very small article, and I am afraid will scarcely be seen by those who are at a distance from the table. This cylinder is mounted horizontally upon a spindle about seven inches long. One of the projecting ends of the spindle has a screw cut upon it of 64 threads to the inch, the other end being left plain. The spindle revolves, in a

similar manner to that of a phonograph, in two brass bearings, the distance between which is equal to twice the length of the



X, Transmitting instrument.
Y, Receiving instrument.
B′, Transmitting battery.
B, Local battery.
R, Variable resistance.
G, Galvanometer.
H, Pin-hole.
S, Selenium cell.
L, Focussing lens.
W, E, Line wires.

cylinder; and one of the bearings has an inside screw cut upon it corresponding to that upon the spindle. The cylinder is shown at Y in the diagram. An upright pillar fixed midway between the two bearings, and slightly higher than the cylinder, carries an elastic brass arm, fitted with a platinum point which presses perpendicularly upon the surface of the cylinder. To the brass arm a binding screw is attached, and a second binding screw on the stand is joined by a wire to one of the brass bearings. The positive pole of a local battery, B, is connected through a set of resistance coils, R,

with the plantinum point, and the negative pole through a galvanometer, G, with the metal cylinder. If now a piece of paper which has been soaked in a solution of iodide of potassium be wrapped round the cylinder, and the cylinder be caused to rotate, the platinum point will, so long as the current passes, trace a very close spiral line upon the paper. The intensity of this line will depend upon the strength of the current; and when the current is interrupted in any manner the line will be completely broken off, and the paper will retain its original whiteness. By regulating the intensity of the line thus traced, and introducing gaps at the proper places, a picture can be represented upon the paper similar to those produced by Bakewell's form of instrument, but with the addition of half-tones. The strength of the current which generates the line current is varied by the action of the luminous focussed image upon a piece of prepared selenium (a so-called "selenium cell") in the transmitting instrument.

At the transmitting station is a second battery, B′, which is connected by two line wires, W, E (one of which would in practice be replaced by the earth), with the galvanometer, G, and the receiving intrument, Y, so that the current passes through them in the *opposite* direction to that of the current from the local battery, B. If now the two currents passing through the paper are of equal strength, they will neutralise each other, and the platinum point will make no mark when the cylinder rotates. If the current from the local battery is stronger than that from the other, the usual brown mark will appear, its intensity depending upon the difference between the strength of the two currents.

In the circuit of the battery, B′, is introduced the transmitting instrument, X, the purpose of which is to regulate the current in such a manner as to produce the desired effect. The current is caused to pass through a selenium cell, S, enclosed in a small box from which all light is excluded except such as can pass through a pin-hole drilled in the side of the box opposite to the cell. By means of a simple mechanical arrangement the box is connected with a horizontal spindle in such a manner that each revolution of the spindle causes the box to move perpendicularly up and down through a distance of two inches, and at the same time laterally

through $\frac{1}{64}$ inch. I am afraid that the arrangement cannot be seen from any distance, but the box is now actually performing the movement.

A picture not more than two inches square being projected by a photographic lens upon that side of the box which contains the pin-hole, H, it is clear that, by turning the spindle, the pin-hole may be caused to pass successively over every point of the focussed image. The box, while moving in the upward direction, travels through space at precisely the same speed as any point on the surface of the receiving cylinder, when the spindles of the two instruments are revolving synchronously: the downward movement is rapid, and has nothing to do with the transmission of the picture.

The instrument is prepared for work in this manner:—A small piece of paper is soaked in a solution of iodide of potassium, and fastened with a little gum along the edges upon the cylinder of the receiver, and the platinum point pressed gently upon it. The pin-hole in the transmitter is brought to the brightest part of the focussed picture, and the variable resistance, R, in the circuit of the local battery is so adjusted that the two currents passing through the paper exactly balance each other, as indicated by the galvanometer, G. The cylinder and box are then moved to their extreme positions, and all is ready to commence operations. The two spindles are then caused to rotate [illustrating] slowly and synchronously. The pin-hole in the course of its up and down path passes successively over every part of the projected picture, and the amount of light falling at any moment upon the selenium cell is proportional to the illumination of that particular spot of the picture which for the time being is occupied by the pin-hole. When the pin-hole is in the dark, the resistance of the selenium cell is increased, the current from the local battery predominates, and the platinum point traces a dark brown line upon the paper. But when the pin hole happens to be passing over a bright part of the picture, the resistance of the selenium cell is diminished; a stronger current is then opposed to that from the local battery, and the line traced by the point is enfeebled or altogether broken. The close spiral line thus described, with the breaks in its uni-

formity, constitutes a picture which, if the instrument were perfect, would be a monochromatic counterpart of that projected upon the transmitter.

Nearly all the defects in the performance of the instrument are due to want of sensitiveness in the prepared paper, which requires a comparatively strong current to mark it. If the electricity could be made to play the same part as light does in the rapid processes of ordinary photography, producing an invisible decomposition capable of subsequent development, there would seem to be no reason why great perfection should not be obtained. No doubt, too, the selenium cells could be much improved, though with a sufficiently sensitive paper they would work very well in their present condition.

In the experimental apparatus synchronism has been secured by the simple expedient of connecting the two spindles and turning them together. For practical use each instrument would of course be driven separately by clockwork, which might be synchronised by an electro-magnetic arrangement at every revolution: since the current from the transmitting battery could be used for this purpose during the descent of the box, no additional line wire would be required.

I am not able to show the apparatus before you in actual operation, because there has not been time to adjust it properly since the meeting of the Congress in this room this afternoon; and even if it were in order it is so small that very few could see it. I hope, however, to have the apparatus on view in the Exhibition in the course of next week.

A vote of thanks was passed to Mr. Shelford Bidwell for his communication.

Mr. AYLMER then read the following paper:—

LES CHEMINS DE FER ELECTRIQUES.

Par Dr. WERNER SIEMENS (Foreign Member).

Les progrès considérables et multiples réalisés dans les applications industrielles de l'électricité, et dont les spécimens sont réunis dans cette remarquable exposition due à l'initiative et à

l'hospitalité de la France, me font un devoir de ne pas absorber pendant trop longtemps votre attention pour une seule de ces nombreuses applications.

Toutefois, je me rends volontiers au désir que m'en à manifesté la " Society of Telegraph Engineers and of Electricians," dont j'ai l'honneur d'être membre dès sa formation et qui a pris une part si active à ces progrès, en vous faisant une courte communication sur la locomotion électrique.

La locomotion électrique, dont le petit chemin de fer conduisant au palais de l'industrie est un spécimen, et qui se présente à vous sous la forme d'un omnibus transportant environ 50 personnes, mis en mouvement par la force électrique, est basée sur la transmission de force au moyen de machines dynamo-électriques.

Je n'ai pas l'intention d'étudier l'origine et les perfectionnements des machines dynamo-électriques et d'entrer dans les détails de leur construction, je me bornerai seulement à appeler votre attention sur certaines propriétés de ces machines, propriétés qui les rendent particulièrement aptes au service de la locomotion.

Les machines dynamo-électriques se distinguent essentiellement des machines à induction magnéto-électriques par l'absence de tout aimant permanent.

Dans les machines basées sur l'emploi d'aimants permanents, la force du courant électrique fourni par la machine dépend de l'intensité constante des champs magnétiques à travers lesquels on fait passer les conducteurs ou les pôles des électro-aimants induits.

Dans les machines dynamo-électriques, au contraire, les aimants permanents des anciennes machines magnéto-électriques sont remplacés par un ou plusieurs électro-aimants dont les spires font partie du circuit général.

L'intensité du champ magnétique efficace dans la machine dynamo-électrique n'est donc pas constante; elle n'est qu'une fonction de l'intensité du courant dont elle même est la cause première.

Si dans une telle combinaison, les masses et les résistances sont convenablement établies, une quantité minime du magnétisme retenue par les électro-aimants suffira pour produire dans les

conducteurs—mus dans ce faible champ magnétique un courant susceptible d'accroître le magnétisme de l'électro-aimant. Le magnétisme ainsi renforcé, augmente à son tour le courant induit, et ainsi de suite, jusqu'à ce qu'un équilibre dépendant de la construction de la machine et de la vitesse du mouvement de rotation se soit établi.

Le magnétisme et le champ magnétique défini par Faraday, le grand investigateur de l'induction, ne forment donc dans la machine dynamo-électrique qu'un effet transitoire, ou si on veut, un produit intermédiaire de la transformation du travail mécanique en courant électrique.

C'est pour cela qu'en décrivant dans la séance de l'Académie des Sciences de Berlin, du 17 Janvier, 1867, le principe dynamo-électrique, j'ai proposé d'appeler les machines basées sur ce principe des machines "dynamo-électriques" pour les distinguer des machines magnéto-électriques qui produisent le courant électrique au moyen de magnétisme existant.

Il est évident, que les machines dynamo-électriques jouent aussi le rôle de machines électro-magnétiques si elles sont parcourues par un courant d'une autre source, comme par exemple, celui produit par une autre machine dynamo-électrique.

Cela résulte directement du principe dynamo-électrique qui exige qu'une machine électro-magnétique quelconque que l'on fait tourner en sens contraire de sa rotation, renforce le courant électrique qui la traverse, c'est-à-dire, constitue une machine dynamo-électrique.

Quoique une machine magnéto-électrique combinée avec une machine électro-magnétique convienne parfaitement en théorie à la transmission de force et plus particulièrement à la locomotion électrique ; en pratique elle ne peut pas servir dans ce but.

D'abord, le magnétisme de l'acier est excessivement faible comparé à l'électro-magnétisme et cela entraîne à donner aux machines magnéto-électriques de très-grandes dimensions s'il s'agit de produire de forts courants, tels que les exige la locomotion. La réunion d'un aussi grand nombre d'aimants d'acier a pour conséquence de leur faire perdre, à bref délai, une grande partie de leur magnétisme. Mais, abstraction faite de ces inconvénients des

machines magnéto-électriques, le fait que, dans la machine dynamo-électrique, le magnétisme est une fonction de l'intensité du courant, forme une condition d'une importance capitale pour les applications du courant électrique à la locomotion.

Si deux machines de ce genre se trouvent placées dans le même circuit et si l'on fait tourner l'une des deux par une force extérieure avec une vitesse déterminée pendant que l'autre aura un travail variable à accomplir, le travail produit par cette machine secondaire, en des limites très-larges, restera presque indépendant de sa vitesse de rotation.

Cela vient de ce que la machine secondaire mise en mouvement par le courant, agit elle-même comme une machine productrice de courant ou comme une machine dynamo-électrique. Elle produit un contre-courant qui affaiblit le courant existant dans le circuit, de telle sorte que le courant tomberait à zéro, si la machine secondaire n'avait aucun travail intérieur ou extérieur à accomplir, et que par suite, le contre-courant qu'elle développe, viendrait à être égal au courant de la machine primaire maintenue en rotation uniforme.

Si maintenant, par un travail imposé à la machine secondaire, on réduit sa vitesse et par conséquent, la contre-action développée par elle, l'intensité du courant augmente dans tout le circuit, et par suite, le magnétisme efficace des deux machines se renforce, d'où il résulte que l'effet dynamique de la machine secondaire se trouve accrû et approche de sa valeur antérieure.

Cette particularité des machines dynamo-électriques placées dans un circuit commun est de la plus haute importance au point de vue des chemins de fer électriques, attendu qu'elle constitue un moyen de régler d'une façon très-parfaite la vitesse de la locomotive électrique ou de "l'Electromote" comme je propose d'appeler une voiture traînée par l'électricité.

C'est à cette propriété des machines dynamo-électriques accouplées que nous devons la force relativement grande avec laquelle l'Electromote démarre, qu'il passe par des courbes, franchit des rampes et peut vaincre d'autres obstacles qui s'opposent à sa marche, sans variations gênantes de vitesse et que cette vitesse n'implique aucun coefficient préjudiciable sur des plans inclinés.

C'est encore cette propriété autorégulatrice des machines dynamo-électriques accouplées qui seule permet la circulation simultanée de plusieurs électromotes sur une même voie, les uns à la suite des autres.

Or, si l'on intercale dans le circuit commun une forte machine primaire, que l'on fait tourner à vitesse constante et si les deux rails conducteurs, comme ou les emploie pour le chemin de fer électrique de Lichterfelde, reçoivent de la machine génératrice une différence de potentiel dépendante de l'intensité du courant dans le circuit conducteur, cette différence de potentiel devra augmenter du moment que la résistance totale du circuit se trouvera diminuée par l'intercalation de deux ou de plusieurs machines associées parallèlement (en quantité).

Si la résistance des rails est petite, comparativement à celle des machines, deux ou plusieurs machines secondaires, d'une construction semblable, et ayant à vaincre des résistances pas trop différentes entre elles, marcheront l'une après l'autre avec une vitesse à peu près semblable.

Cette vitesse ne sera pas même bien inférieure à celle qu'acquerrait une seule de ces machines secondaires circulant sur la ligne.

Si l'un des électromotes devait vaincre une résistance plus grande que les autres, sa vitesse diminuerait, et par conséquent également le contre-courant qu'elle produit.

Son aimant sera donc parcouru par un courant plus fort et par cela l'effet mécanique de cet électromote se trouvera renforcé.

Il en résulte que la vitesse de plusieurs électromotes circulant sur une même voie, les uns à la suite des autres, ne différera pas beaucoup, même pour des chargements différents et pour des résistances variables rencontrées sur la voie.

Ces considérations prouveront que pour l'exploitation des chemins de fer électriques, ne conviennent que des machines dynamo-électriques simples, à parcours unique de courant, telles que la machine Gramme ou celle de Von Hefner Alteneck.

Ni la combinaison dynamo-électrique que M. Wheatstone a indiquée quelques semaines seulement après que j'avais publié le principe dynamo-électrique, ni les machines à excitatrice indépendante ne conviennent au but proposé.

Quoiqu'immédiatement après la découverte du principe dynamo-électrique, j'avais songé à l'application des machines dynamo-électriques au service de la locomotion, dix anneés se sont cependant écoulées avant que j'aie osé entamer l'essai pratique d'installer un chemin de fer électrique.

Mes premières machines dynamo-électriques basées sur l'emploi de l'électro-aimant qu'on est convenu d'appeler l'armature Siemens ne donnaient pas un rendement suffisant de transmission de force, et présentaient cet autre inconvénient que le fer de l'armature s'echauffait trop fort.

C'est à M. Gramme qu'est dû le mérite d'avoir, en faisant usage de l'anneau Paccinotti, appliqué le premier le principe dynamo-électrique sous une forme susceptible d'un fonctionnement prolongé.

Il ne devint possible de construire des chemins de fer électriques, qu'après que la machine Gramme d'une part, et la machine conçue par le chef de notre bureau d'études M. Von Hefner Alteneck d'autre part, cette dernière exclusivement appliquée par notre firme, eurent affecté une forme pratique bien définie.

Dans le premier spécimen de chemin de fer électrique, celui qui, au printemps 1879, fonctionnait sur le territoire de l'exposition industrielle de Berlin, le circuit électrique était établi par un troisième rail spécial, isolé des deux rails de roulement et placé entre ces derniers ; les rails de roulement constituaient le fil de retour non isolé.

Nous avons construit une petite locomotive spéciale dont les roues tournaient par l'effet d'une machine dynamo-électrique les actionnant par l'entremise d'engrenages.

Au moyen d'un appareil à contact, l'une des extrémités des spires de la machine électrique était reliée au rail central isolé, alors que l'autre extrémité communiquait avec le chassis de la locomotive et par cela même avec les rails de roulement.

La voie était circulaire, d'une longueur d'environ 300 mètres et de 0·50m. d'écartement.

La machine dynamo-électriqne primaire installée dans la galerie des machines était intercalée entre le rail central isolé et les rails extérieurs de roulement.

La locomotive remorquait sur cette voie trois petits chars-banc à voyageurs occupés par 18 personnes et cela à une vitesse de 3 à 4 mètres par seconde.

Des petits chemins de fer de ce genre fonctionnent en assez grand nombre en divers endroits.

Dans le premier chemin de fer électrique, construit pour une exploitation régulière et inauguré au mois de Mai de cette année-ci à Lichterfelde près de Berlin, on a fait usage d'un système de conducteurs différents.

Le troisième rail central a été supprimé et les deux rails de roulement fixés sur traverses en bois, et par ce fait, isolés l'un de l'autre forment le fil d'aller et le fil de retour.

Un type de voiture employé sur la ligne de Lichterfelde est exposé dans la section allemande de l'exposition.

Les roues sont à plateau en bois et les bandages qui, par leur contact avec les rails, amènent le courant aux extrémités des spires de la machine secondaire se trouvent par ce fait isolés du chassis en fer de la voiture.

On n'a pas constaté, jusqu'ici, pour cette ligne longue d'environ 3 kilomètres un effet nuisible quelconque provenant de l'humidité du sol.

L'effort exercé par la machine secondaire fonctionnant à une vitesse d'environ 40 kilomètres à l'heure est approximativement de 5 à 6 chevaux-vapeur.

Le renversement de la marche se produit par un changement des communications électriques entre les fils de la partie tournante de la machine secondaire et ceux de l'électro-aimant fixe.

C'est de cela que dépend le sens de rotation de la machine dynamo-électrique.

Afin d'éviter de fortes étincelles nuisibles à la conservation des machines, l'interruption du courant et la commutation du sens de rotation n'ont pas lieu subitement, mais successivement.

On emploie pour cela un commutateur que l'on fait tourner à l'aide d'un levier, et qui sert à interposer successivement dans le circuit des résistance de plus en plus grandes.

Ce chemin de fer a principalement été construit dans le but de recueillir des données pratiques d'expériences pour le chemin de fer aérien projeté par nous pour Berlin.

La disposition adoptée à Lichterfelde ne convient pas pour des rues dans lesquelles circulent des voitures traînées par des chevaux, attendu que ces animaux s'effraient toutes les fois qu'ils reçoivent des commotions électriques, leurs sabots venant toucher les deux rails à la fois.

Il est donc nécessaire, pour les chemins de fer placés dans ces rues, de ne pas transmettre le courant par les rails, mais d'employer à cette fin des conducteurs suspendus séparément, comme le fait voir le tramway électrique de l'exposition et comme cela a été fait récemment pour le tramway électrique de Charlottenburg à Spandau.

Pour ce dernier tramway, le courant est conduit par deux câbles métalliques isolés entre eux, suspendus parallèlement de telle façon à servir eux-mêmes de voie de roulement à un petit chariot à contact.

Les roues de droite du chariot sont isoleés de celles de gauche et établissent au moyen d'un léger câble à deux conducteurs séparés la communication entre les câbles conducteurs fixes et les spires de la machine dynamo-électrique placée sous la voiture.

Pour le tramway électrique, qui a été construit à Paris, on a appliqué une disposition différente.

Deux tubes métalliques d'environ 0·02m. de diamètre intérieur, à l'instar du tablier des ponts suspendus, ont été suspendus latéralement à la voie au moyen de forts fils isolés.

Chacun de ces tubes est muni d'une fente longitudinale à travers laquelle un piston métallique mobile, se mouvant dans chacun de ces tubes, établit la communication conductrice avec la machine de propulsion.

Le contact parfait entre piston et tube est assuré au moyen de galets à friction.

Le courant émis par la machine dynamo-électrique primaire parcourt les deux tubes isolés l'un de l'autre et est amené à la machine de l'électromote par les pistons entrainés à la remorque de la voiture.

Une troisième méthode pour le maintien constant de la liason conductrice entre la machine primaire et la machine secondaire mobile consiste à disposer entre ou à côté des voies un câble métal-

lique conducteur reposant sur des appuis isolés, et lequel, au passage de l'électromote, est relevé et remis en place au moyen de galets à l'instar de ce qui se fait pour les chaînes des bateaux de touage.

Cette méthode, jusqu'ici, n'a pas encore été mise en pratique.

L'expérience acquise par les chemins de fer électriques installés jusqu'ici a fourni la preuve que la locomotion électrique est pratiquement réalisable et qu'elle conquérera un champ d'applications déterminé, dans lequel, elle rendra des services meilleurs que ceux obtenus par les moyens de transport utilisés précédemment.

Je tiens à déclarer ici d'avance que je ne suis pas d'avis que l'électromote supplantera jamais la locomotive à vapeur, pas plus qu'il ne remplacera partout les chevaux.

L'indépendance à se mouvoir que possède la locomotive à vapeur lui assurera toujours une grande prépondérance pour le transport de masses considérables sur de longs parcours.

La locomotive électrique ne trouvera sa place légitime que là ou le locomotive à vapeur, à cause de ses particularités, ne pourra convenir, ou encore, lorsque des conditions spéciales favoriseront l'application de la traction électrique.

Nous citerons comme particulièrement avantageux à être exploités par traction électrique, les chemins de fer exhaussés (elevated railroads) des grands centres populeux.

L'extension des villes entraîne à un accroissement si considérable de la circulation dans les rues que l'espace indispensable à cette circulation commence à manquer. Du moment qu'il y a empêchement à créer une voierie souterraine pour la communication rapide des faubourgs entre eux et du centre avec les confins, on devra nécessairement établir un réseau de chemins de fer aériens, si la vie ne doit pas être rendue insupportable et le développement ultérieur se trouver arrêté par un engorgement dans la circulation.

Les chemins établis sur colonnes comme en Amérique sont le moyen le plus simple pour l'obtention d'un second étage de voierie pour la circulation rapide et le dégrèvement de l'encombrement des rues.

La traction à vapeur a fortement discrédité les elevated railroads, attendu que, selon les exigences de la traction à vapeur, on

a dû tenir compte, dans leur construction, des poids considérables des locomotives et des lourds trains à remorquer.

A cela vient s'ajouter la fumée et la poussière projetées par les locomotives et le bruit désagréable pour les riverains.

Avec la traction électrique au contraire, la voie n'a besoin d'être construite que pour des véhicules pesant relativement peu et roulant isolément sur des colonnes d'un diamètre peu supérieur à celui des réverbères.

Ces voitures se succéderaient à petits intervalles et seraient à peine remarquées par les riverains, lorsque, lancées à la vitesse des trains de chemin de fer, silencieusement, elles voleraient vers leur destination.

Il est vrai que de semblables chemins de fer sur colonnes ne contribueront pas à l'embellisement des rues, mais d'autre part, on s'habitue vite à ce qui est utile et nécessaise.

C'est ainsi que le téléphone a réussi à avoir raison de l'antipathie qui existait précédemment contre l'enlaidissement de l'aspect des rues par une épaisse toile de conducteurs aériens.

De même, on remplacera la circulation lente sur le niveau des rues en bien des cas par les moyens de transport électriques à conducteurs suspendus, attendu que, la traction électrique est bien moins coûteuse que ne l'est celle par les chevaux et que d'autre part, la rue est ainsi en quelque sorte dégrèvée d'un excès de circulation.

La traction électrique convient encore particulièrement pour l'exploitation de lignes secondaires économiques et de tronçons de raccordement, ainsi que pour les transports pondéreux à effectuer dans des usines et dans les mines.

Le halage des barques sur les canaux dans l'avenir se fera à l'électricité et on trouvera encore de nombreuses autres applications pour lesquelles la traction électrique offrira des avantages sur la traction à vapeur ou par chevaux.

L'exploitation des lignes souterraines et des longs tunnels, tels que celui du St. Gothard, anisi que le tunnel sous la Manche destiné à relier l'Angleterre à la France une fois achevé, vraisemblablement ne se fera pas autrement qu'à l'électricité.

Il est vrai que, pour le moment, la moitié environ du travail dépensé est perdue dans la traction électrique.

Cette perte, toutefois, n'est qu'apparente, attendu qu'avec de puissantes machines à vapeur fixes, la dépense du combustible et comme quantité et comme prix est bien inférieure à la dépense occasionnée de ce chef par une locomotive de même force.

La où des forces hydrauliques naturelles, généralement utilisées jusqu'ici d'une façon incomplète dans les pays montagneux, peuvent servir à activer la machine primaire, aucun autre moyen de traction ne pourra concourrir avec la traction électrique. Dans ces conditions elle constituera avec un immense succès un véritable service de renfort et permettra d'utiliser la force non encore employée que charrient les eaux découlant des hauteurs pour y élever, sans frais ancun, à vrai dire, les hommes et les marchandises.

Dr. C. Wm. Siemens : Permettez moi de dire quelques mots sur la communication de mon frère, que nous venons de recevoir. Je crois que ce mémoire indique assez clairement les limites dans lesquelles mon frère pense que le chemin de fer électrique peut être appliqué. Les trois applications que nous avons déjà faites, et dont il parle, présentent entre elles des différences essentielles. Dans l'une, la première, il y a un conducteur en forme de rail situé entre les deux rails sur lesquels les wagons circulent, ces derniers servant de conducteurs de retour. Cette disposition présente un inconvénient évident en exigeant un troisième rail, lequel doit être assez fort pour résister au trafic ordinaire des rues. L'on a pû modifier cette construction à Lichtenfelde de manière à diriger un courant par le rail d'un côté, et celui de retour par le rail de l'autre côté ; cette disposition, qui forme le second système, me paraît la plus convenable pour les chemins élevés, où il est possible de maintenir les deux rails propres et d'obtenir ainsi un contact assez parfait entre les roues et les rails, mais il necessite une modification dans la construction du matériel de roulement, en ce sens que les roues d'un côté et de l'autre du wagon moteur doivent être isolées les unes des autres, c'est-à-dire que les deux roues d'un côté doivent être construites en une matière isolante, telle que le bois par exemple. Pour l'application que nous avons faite ici à l'exposition, et à laquelle j'ai pris quelque part, il aurait été impossible

d'employer l'une ou l'autre de ces dispositions à cause du trafic considérable des rues, et de la boue qui empêcherait le contact entre les roues et les rails; c'est ce qui a amené la nécessité d'employer deux conducteurs aériens. Cette disposition, qui forme le troisième système, a été exécutée par M. Boistel, le représentant de notre maison à Paris. Mon frère a appliqué une construction semblable à Charlottenbourg, où je crois que les wagons ne rouleront pas sur des rails; du moins les rails ne sont pas absolument nécessaires quoiqu'en les supprimant l'on aurait plus de resistance à vaincre. Touchant la perte de force en transmission, dont le mémoire parle, quoique cette perte soit assez considérable, pouvant s'élever entre 40 et 60 pour cent., cependant l'avantage d'avoir une source de force en une machine fixe est très considérable. Je ne parle pas des machines pour les grandes voies, pour lesquelles une machine locomotive peut être très économique, et presque aussi économique que les bonnes, je ne dis pas les meilleures, mais de très bonnes machines fixes. Il en est autrement cependant pour les petites machines que seules l'on peut employer pour les tramways; là les dépenses de combustible, les difficultés d'avoir une petite chaudière effective, et les inconvénients, causés par l'échappement de la fumée et de la vapeur dans les rues, sont tels que les tramways à vapeur n'ont jamais pû réussir; on les a essayés de côtés et d'autres mais ils ont toujours finalement manque à cause de ces difficultés, et la transmission électrique paraît offrir une solution pratique.

Mon frère a peur que les petits chariots à contact ne tombent des fils suspendus.

En vérité c'est arrivé au commencement de nos expériences et il a fallu construire les chariots bien soigneusement pour obvier à cet inconvénient, et pour les faire passer aisément par les courbes. Certainement la méthode employée ici, à Paris, par mon frère et M. Boistel a des avantages considérables, mais elle est aussi plus coûteuse.

M. le docteur Werner Siemens: Je voudrais seulement ajouter quelques mots. Mon frère disait que ces chariots devraient tomber de la hauteur de ces deux fils; mais c'était en vérité au commencement le cas, parce que c'était un chariot avec quatre roues fixes.

Il faudrait changer tout à fait l'espèce de ces chariots et faire les axes mobiles de manière que tout cela pût changer et se plier et s'adapter à cette corde; en sorte que le contact est toujours juste et que cela ne dévie ni d'un côté ni de l'autre. Avec ce moyen, on marche assez sûrement, et il est tout à fait impossible que ces chariots tombent de haut en bas, parce qu'il y a des arrangements que cela est tout à fait impossible. Mais cette manière comme elle est employée par mon frère et par M. Boistel ici à Paris est plus certaine. Seulement c'est aussi un peu plus chère, et je demande ce qui sera le mieux.

M. le PRESIDENT : Si personne n'a plus d'observations à faire, nous allons entendre M. Govi qui nous fera l'honneur de nous communiquer ses impressions sur la machine de l'honorable M. Pacinotti.

L'ANNEAU DE M. PACINOTTI.

Par M. le professeur GOVI.

Mesdames et messieurs,—Je vous remercie d'abord de l'intérêt que vous paraissez attacher à l'invention d'un de mes compatriotes, et je remercie le Société des Ingénieurs Télégraphiques de l'honneur qu'elle a voulu me faire en m'appelant ici à être le porte-voix de M. Pacinotti.

M. Pacinotti a imaginé sa machine lorsqu'il était trés jeune; il n'avait que 19 ans quand il l'a inventeé; ce qui n'enleve, ni n'ajoute rien au mérite de la machine, mais ce qui peut ajouter quelque chose au mérite de l'inventeur. Voila pourquoi je tenais à vous faire remarquer cette circonstance. La machine de M. Pacinotti est une machine magnéto-électrique, mais elle est en même temps une machine motrice, et il faut même dire que la première idée de son inventeur a été d'en faire un moteur électrique. Dans les anciens moteurs imaginés par différents savants, des électro-aimants passaient devant des aimants fixes, qui les attiraient ou les repoussaient et il s'ensuivait un mouvement qui était transmis à différents organes. Quelquefois c'était le contraire qui avait lieu; mais il y avait dans tous le cas de fréquentes inversions de courants, des contacts brûlés et beaucoup de perte de force.

M. Pacinotti se demanda si on ne pouvait pas obtenir le mouvement d'une façon continue, sans interruption, sans saccades, et ce fut là le point de départ de son invention.

Pour y réussir, il songea à prendre un anneau de fer à section circulaire, un cylindre recourbé en anneau et à enrouler sur ce cylindre recourbé en anneau un fil de cuivre revêtu de soie, de manière à former un circuit entièrement fermé à la surface de l'anneau. Si, dans ces circonstances-là, on introduit un courant dans ce circuit fermé par deux points assez rapprochés du fil, on a une espèce d'aimant, qui n'en est pas un, dont les pôles se succèdent deux à deux en se touchant, et qui n'a par conséquent pas d'action extérieure. Mais si, au lieu d'introduire le courant électrique et de le faire sortir par deux points très rapprochés du circuit qui s'enroule autour de cet anneau, on l'introduit et on le fait sortir par deux points diamétralement opposés, alors l'aimantation qui se produit engendre dans l'anneau, aux deux points par où le courant entre, et par où le courant sort, deux pôles opposés : d'un côté un pôle austral, de l'autre côté un pôle boréal, et l'anneau peut être considéré comme formé de deux aimants semi-circulaires présentant chacun à l'aimant voisin le pôle du même nom : deux pôles austraux d'un côté, deux poles boréaux de l'autre, qui se touchent. Voila quel est le point de départ de la machine de M. Pacinotti, machine, du reste que vous voyez ici et que je ferai fonctionner tout à l'heure en alimentant le mouvement de cet anneau à l'aide de cette autre machine magnéto-électrique (également de M. Pacinotti) qui peut fournir des courants au premier appareil.

L'anneau avec son circuit peut tourner librement autour d'un axe normal à son plan et passant par son centre.

Maintenant que nous avons cet anneau ayant un pôle en un certain point, et un autre pôle au point diamétralement opposé, si on place à 90° de ces pôles, ceux d'un aimant permanent, ou d'un électro-aimant fixe, on verra l'anneau tourner pendant un instant, puis le mouvement s'éteindra, parce que le passage du courant aura été interrompu par la rotation même de l'anneau.

C'est pour cela que M. Pacinotti a imaginé de partager la spirale qui enveloppe l'anneau, en plusieurs bobines succesives, de telle

façon toutefois, que la fin d'une des bobines se rattachât au commencement de l'autre, et qu'en réalité l'ensemble de ces bobines constituât une spirale continue, toujours dirigée dans le même sens, mais dont, de temps en temps, un de tours s'allonge pour déscendre le long de l'axe qui supporte l'anneau, et se rattacher à des touches, ou contacts, en cuivre, où les deux bouts du fil sont soudés, et contre lesquels peut s'appuyer un contact fixe, destiné soit à transmettre de l'électricité, soit à en recevoir ; à transmettre de l'électricité quand il s'agit de faire de cet appareil une machine motrice; à recevoir de l'électricité quand il s'agit d'employer cette machine comme source d'électricité. Les contacts fixes touchent le commutateur, qui porte les fils prolongés des bobines, en deux points diamétralement opposés. Voila donc la machine toute prête à fonctionner.

Car, si on fait passer un courant électrique par les contacts placés à la partie inférieure de l'axe (par le *commutateur*), et si l'on fait entrer ainsi ce courant électrique dans les bobines de l'anneau, on vient par là-même à faire naître deux pôles dans cet anneau diamétralement opposés l'un à l'autre. Or, si, en croix avec la position de ces 2 pôles, vous imaginez un aimant fixe, un aimant permanent, ou bien un électro-aimant assez fort pour pouvoir, en agissant sur les pôles de l'anneau déterminer un mouvement dans l'anneau et dans l'axe qui le supporte, c'est-à-dire, dans la machine libre ou rattachée à d'autres organes de mouvement et de travail, vous comprenez que le pôle austral de l'aimant permanent, ou de l'électro-aimant, en attirant le pôle boréal de l'anneau, le fera tourner de son côté, et en même temps, en repoussant le pôle austral de l'autre côté, tendra à faire tourner l'anneau dans le même sens. L'autre pôle en agira de même, et l'anneau se mettra à tourner autour de l'axe vertical, et le mouvement de le machine sera obtenu.

Mais comme toutes les bobines n'en font qu'une, et qu'elles reproduisent par conséquent toujours aux mêmes points opposés de l'anneau les mêmes pôles, on a ainsi deux couples qui agissent continuellement de la même façon, sans interruption pour faire tourner l'anneau toujours dans un même sens. Voilà en très peu de mots le principe d'après lequel M. Pacinotti a construit sa machine, en tant qu'elle doit fonctionner comme machine motrice.

Mais Monsieur Pacinotti ne s'en est par tenu là, et il a imaginé à la même époque, il a même suggéré dans son mémoire de 1864, d'ajouter aux pôles des aimants fixes deux prolongements en fer, qui embrassent une grande partie de l'anneau ; de telle façon que, lorsque le mouvement se produit, les pôles de l'anneau commencent à ressentir plus tôt l'influence des pôles de l'aimant fixe, et restent plus longtemps sous leur influence. Le mouvement de la roue tournante acquiert comme cela plus d'énergie, et la machine peut fournir un travail plus considérable. M. Pacinotti, peu de temps après avoir imaginé cette disposition de machines motrices, fit plusieurs expériences pour évaluer le travail que pouvait fournir sa machine, et il trouva, en se servant de freins, et d'un voltamètre, que son appareil rendait au moins autant que la meilleure machine motrice électro-magnétique connue jusqu'alors.

Toutes ces recherches et ces expériences furent exécutées par M. Pacinotti entre 1860 et 1864 époque à laquelle il fit paraître la description et la figure de sa machine dans le *Nuovo Cimento.*

Dans cette description, après avoir indiqué la construction de son moteur électrique, M. Pacinotti a ajouté une remarque qui est peut-être la partie la plus intéressante de son mémoire. C'est la remarque relative à la transformation de sa machine motrice en machine productrice d'électricité, car c'est comme machine productrice d'électricité surtout que cet appareil a acquis plus tard une si grande importance.

J'ai oublié de dire tout à l'heure que M. Pacinotti, au lieu de laisser, comme il l'avait imaginé d'abord, son anneau de fer lisse et uni, avait, imaginé dès le commencement d'en faire saillir des espèces de dents en fer, entre deux desquelles il enroulait le fil de chacune des bobines faisant le tour entier de l'anneau. De cette manière il obtenait de rapprocher davantage des pôles de l'aimant fixe le fer qui devait en subir l'induction, ou bien qui devait être attiré par ces pôles, lorsque l'anneau avait été magnétisé d'abord par l'action d'un courant.

C'est la disposition que vous voyez dans cette roue, la même qui a été construite par M. Pacinotti en 1860.

La transformation de cette machine motrice en machine productrice d'électricité est fort simple.

On n'a pour cela qu'a renverser l'opération. Au lieu d'introduire un courant par les contacts de l'interrupteur et de faire naître ainsi deux pôles fixes aux extrémités d'un diamètre de l'anneau, lesquels pôles doivent être continuellement attirés ou repoussés par les deux pôles de l'aimant permanent ou de l'électro-aimant fixe placé sur les côtés de la roue, au lieu de faire cela, supposez qu'a l'endroit où l'on aurait attaché les deux fils provenant de la pile, on attache deux fils destinés à conduire l'électricité à l'endroit où on veut l'utiliser, à une lampe électrique, par exemple, et supposez qu'au lieu de charger l'aimant fixe d'imprimer le mouvement à l'anneau, on fasse tourner cet anneau par un moyen mécanique, en le mettant en communication avec une machine motrice. Qu'arrivera-t-il dans ce cas? Les deux pôles de l'aimant permanent, ou de l'électro-aimant fixe tendront à produire vis-à-vis d'eux deux pôles contraires, dans l'anneau de fer qui va tourner devant-eux. Ces deux pôles contraires seront nécessairement fixes dans l'espace; quand l'anneau tournera ils ne se déplaceront pas (du moins pour de petites vitesses), parce qu'ils ne sont dûs qu'à l'induction exercée par les pôles de l'aimant fixe, qui ne changent point de place. Mais les bobines entraînées par la rotation de l'anneau glisseront sur ces pôles immobiles, et prendront des états électriques qui pourront être transmis aux fils conducteurs qui aboutissent aux touches du commutateur. . . . . . .

Pendant que je me repose un instant, je prie M. Pacinotti, que j'ai l'honneur de vous présenter, de vouloir bien mettre en mouvement son appareil comme appareil moteur, à l'aide de cette autre machine magnéto-électrique, qui transmettra un courant continu à l'anneau magnétique ou *aimant tranversal* que vous avez sous les yeux.

M. Govi (après avoir dessiné un anneau sur le tableau noir): Supposons, messieurs, que ce soit là l'anneau en fer de M. Pacinotti et que, sous l'action des deux pôles d'un aimant permanent mis à côté de cet anneau il se développe d'un côté un pôle que j'appellerai "nord," du côté opposé un pôle que j'appellerai "sud." Si maintenant, on fait tourner rapidement cet anneau, ses deux pôles ne changeront pas de place, ils resteront toujours là; mais si sur cet anneau on a disposé différentes bobines qui se suivent, et sont

toutes jointes ensemble de manière à ne former qu'un seul circuit, il est évident que ces bobines glisseront, pour ainsi dire, sur les pôles qui restent fixes ; chacune des bobines se rapprochant tour à tour d'un pôle, passant par dessus, le dépassant et allant joindre le pôle opposé.

Or, quand on fait glisser une courte bobine, sur un aimant, en partant de la ligne moyenne ou neutre de cet aimant et en allant vers son pôle austral ou vers son pôle boréal, il se développe dans cette bobine un courant dont la direction se détermine très facilement, quand on connait la direction des courants imaginés par Ampère dans l'intérieur des aimants. Ainsi le pôle d'un aimant qui se tourne vers le sud, a son courant dirigé de manière à tourner comme l'aiguille d'une montre ; la direction est de sens contraire pour le pôle qui se dirige vers le nord. Eh bien, si on fait glisser une courte bobine sur cet aimant à partir de sa ligne neutre, il s'y développe un courant dont la direction est opposée à celle du pôle dont elle se rapproche. Si maintenant au delà du pôle que la bobine vient d'atteindre on se représente un autre aimant ayant le pôle du même nom à la suite du pôle sur lequel la bobine est arrivée, et c'est le cas de l'anneau de M. Pacinotti, on aura deux pôles du même nom qui se toucheront et sur lesquels passera en glissant la bobine. Mais aussitôt qu'elle aura dépassé ce double pôle, elle sera parcourue par des courants dirigés dans le même sens que ceux du pôle qu'elle vient de quitter, et par conséquent dirigés comme les courants développés pendant la première moitié de sa course. L'intensité du courant dans la bobine va en croissant à mesure qu'elle approche de l'un des pôles ; quand elle l'a dépassé et que la bobine glisse sur le second aimant le courant continue à avoir le même direction, mais il commence à diminuer jusqu'a ce qu'on arrive au point milieu de ce second aimant, où le courant cesse pour recommencer en s'approchant de l'autre pôle, mais après avoir changé de direction.

Maintenant si dans l'appareil de M. Pacinotti, nous faisons partir une bobine du point milieu entre le pôle nord et le pôle sud, et si nous imaginons que cette bobine glisse le long du cercle, on voit tout de suite qu'elle doit avoir son courant dirigé d'une certaine manière qui restera la même après que la bobine aura dépassé le pôle, jusqu'à ce qu'elle parvienne sur la seconde ligne ou section neutre.

Arrivée là, le courant changera de direction, et en prendra une toute contraire, qu'il conservera quand la bobine dépassera le second pôle et jusqu'à ce qu'elle arrive sur la section neutre, où elle avait commencé son mouvement. Le courant reprendra alors sa direction primitive, et continuera ainsi indéfiniment à marcher dans les deux sens opposés sur les deux moitiés de l'anneau en rotation, en acquérant le maximum d'intensité quand les bobines passeront sur chacun du deux pôles. On peut donc recueillir les deux courants aux deux points opposés du circuit au moyen du commutateur auquel aboutissent les fils de bobines et avoir ainsi dans un circuit extérieur un courant unique ayant partout et toujours la même direction.

Cest ce qu'on peut vérifier même avec ce petit appareil en faisant que les courants se rendent, non pas dans une lampe électrique (le petit appareil ne permettrait pas d'en obtenir de la lumière), mais dans une boussole rhéométrique, dans une petit galvanomètre, dont on voit l'aiguille dévier très fortement et toujours dans le même sens pendant la rotation de l'anneau en présence de l'aimant ou de l'électro-aimant fixe.

Si l'on tourne la roue dans un sens opposé la direction du courant se renverse immediatement. Avec le petit appareil que vous voyez là, ou ne peut pas obtenir de courant très fort; cependant si à la place de ces deux électro-aimants que vous voyez et qui, en ce moment ne fonctionnent que par leur magnétisme résidu on mettait en présence de l'anneau tournant les deux pôles, d'un fort aimant permanent ou d'un puissant électro-aimant, on pourrait en obtenir un courant assez intense pour traverser un voltamètre, décomposer une certaine quantité de sulfate de cuivre, rendre incandéscent un fil fin de platine, etc. Mais ce qu'on peut obtenir en petit, on peut l'augmenter facilement en grandissant l'appareil, en accroissant la force motrice, en augmentant l'intensité d'aimantation des électro-aimants, ou des aimants permanents qui détermient l'action de l'appareil lui même, et l'on peut en obtenir alors tour les merveilleux effets qu'on voit se produire chaque jour dans l'enceinte de ce palais.

C'est de l'apparition de ce modeste appareil qu'on peut dater l'origine de la plupart des machines magnéto-électriques et des

machines dynamo-électriques qui aujourd'hui fournissent si abondamment la lumière, les actions chimiques et la force dans beaucoup d'industries.

Voila, Messieurs, tout ce qui j'avais à vous dire relativement à la machine de M. Pacinotti. Evidemment, elle n'est pas un grand instrument, elle n'est qu'un petit modèle, mais si on doit juger de l'importance et de la valeur d'une invention non pas d'après la dimension de l'appareil, par lequel on l'a realisée d'abord, mais d'après la grandeur des résultats qu'elle a produits, je crois que la petite machine de M. Pacinotti peut bien être regardée comme une grande invention, qui méritait d'être tirée de l'oubli où la modestie de son auteur l'avait laissée jusqu'ici.

Je dois ajouter, messieurs, pour les personnes qui desireraient avoir quelques éclaircissements de plus sur cette question que M. Pacinotti se fera un plaisir de les leur donner.

M. ANTOINE PACINOTTI: J'ai besoin d'exprimer d'abord ma reconnaissance a M. le professeur Govi de ce qu'il a fait pour mettre en évidence ma priorité dans la construction, l'usage, et l'explication de *l'electro-aimant transversal;* et pour m'avoir invité à me rendre ici, où j'ai été reçu avec tant de bienveillance.

Ma reconnaissance je dois la témoigner encore à tous les ingenieurs constructeurs qui ont produit des machines électromagnétiques et magneto-électriques d'après le principe de mes electro-aimants transversaux, et qui'en ont montré de la sorte toute l'importance.

Enfin, je dois exprimer ma reconnaissance à toutes les personnes ici présentes, qui ont bien voulu prendre quelque intérêt, à l'exposition de l'origine et de la théorie de mon electro-aimant transversal.

M. le PRESIDENT a ajouté: Puisque personne ne demande la parole il ne me reste plus qu'à exprimer à M. le professeur Govi et à M. Pacinotti, au nom de la Société des Ingénieurs Télégraphistes et des Électriciens, nos vifs remerciments pour cette communication.

Messieurs, l'ordre du jour étant épuisé, je dois annoncer à nos membres et aux savants étrangers qui ont bien voulu assister aux

séances de notre Société que c'est aujourd'hui notre dernière réunion dans ce local. En même temps, j'ai le plaisir de féliciter nos membres du succès des deux réunions qu'il nous a été permis de tenir à Paris et d'exprimer à MM. les savants étrangers notre reconnaissance pour l'honneur qu'ils nous ont fait par leur présence parmi nous, et surtout aux hommes distingués qui nous ont honoré de leurs communications.

Je ne puis pas terminer sans renouveler nos remerciments les plus sincères à M. Cochery, Ministre des Postes et des Télegraphes, pour l'accueil bienveillant qu'il a accordé à notre société et pour l'empressement qu'il a montré à mettre à notre disposition toutes les ressources de l'Exposition.

Nous tenons aussi à exprimer encore une fois à M. Berger, Commissaire Général de l'Exposition, notre reconnaissance pour le bonne volonté qu'il a sans cesse montrée à la Société tout entière aussi bien qu'à chacun de nos membres en particulier qui a dû faire appel à sa bonté.

Si je nomme en dernier lieu, M. Bréguet parmi ceux auxquels nos remerciments sont tout spécialment dûs, c'est que nous avons l'honneur de le compter au nombre des membres de la Société des Ingénieurs Telegraphistes et des Électriciens.

La séance est levée.

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The Hundred and Third Ordinary General Meeting of the Society was held on Thursday evening, November 10th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. Foster, F.R.S., President, in the Chair.

The minutes of the two Extraordinary Meetings held in the Electrical Exhibition at the Palais de l'Industrie, Paris, were read and confirmed.

The Secretary announced that the Council had transferred

Mr. James Mackenzie

from the class of Associates to that of Members.

The following paper was then read:—

ON A COMPLETE SYSTEM OF SYNCHRONIZING BY ELECTRIC TIME SIGNALS, AS NOW ADOPTED IN LONDON AND ELSEWHERE.

By J. A. Lund (Member).

The subject which I have the honour to bring before you this evening is one which differs materially from those which usually occupy the attention of this Society. It evolves no new discovery of some hitherto unknown principle, and advances in no new direction the existing knowledge of electrical laws; it only professes a

humbler sphere of action, viz., an attempt to demonstrate the results to be obtained from the patient use of a very limited acquaintance with the science of electricity when brought perseveringly to bear upon some practical demand of daily life.

The speaker has been for four years continuously engaged in inventing and perfecting a system for ensuring uniformity of time between our public and private clocks, and he submits his system of time signalling and synchronizing as meeting every practical and reasonable demand for that end.

So much is said at times about "synchronizing" being "no new thing," and it is so manifestly just to past efforts in this direction to acknowledge them, that a short statement on this point is both right and necessary.

The Patent Office could alone fully reveal what other records so abundantly show, that the attention of the earliest electricians was as much devoted to "synchronising" clocks as to message telegraphing; and it was only after repeated failures in the former, attended by as marked success in the latter, that this became the favourite field of the electrician, to the almost entire exclusion of the less promising arena. We have not time, nor is the present the proper opportunity, for following categorically the efforts here referred to; we will, therefore, for the present purpose of a general explanation, divide "electric clocks" into six kinds.

1. Those whose motive power is the electric current, acting upon what is usually the escapement, and impelling the hands. It has no pendulum. Shepherd's patent is a good example. One of them is the well-known dial fixed on the right-hand side of the gates of the Royal Observatory, on Greenwich Hill, and where, as there, *the circuit is very short*, and a close personal attention is given, it is a very efficient electric clock.

2. Those whose motive power is also the electric current, but in which it acts through a pendulum upon the escapement (exactly the reverse of an ordinary clock), and thence *drives* the hands. Mr. Richie's, of Edinburgh, is the best specimen of this mode: several have been erected in the thoroughfares of that city, worked from the Observatory, and have acted well.

3. Those with an ordinary train, with a weight or mainspring

as the source of power, but with the vibrations of the pendulum controlled by the current. This is the plan most generally adopted, and is the subject of several ingenious patents.

4. Those with the ordinary motive power and train work, but regulated to a small gaining rate, the clock being automatically stopped at *its own* hour, minute, and second, but instantly liberated by the current at the corresponding *true* time. A clock of this kind formed for a long time the standard of time at Lombard Street Post Office, the clock being controlled from Greenwich, and reporting itself to the Observatory after every "set" to enable its being checked there. This system shared the fate of others, and was eventually discarded, and the clock removed.

5. Clocks with the ordinary train and motive power, but in which the motion-work (the wheels carrying and regulating the motions of the hands) is acted upon by an electro-magnet, acting by driving a V piece into a notch on the periphery of a wheel. It is not by any means a satisfactory method, and has never, although in different forms the base of at least six patents, been largely adopted.

6. Clocks of the ordinary construction, in which *the hands* are set to Greenwich time by the direct action of the current acting upon them. Bain first attempted this plan; in fact, he claimed (we think justly) to have been the originator of electric clocks: his method was, however, exceedingly crude, and was never adopted in practice.

The above may be roughly divided again thus—clocks *driven* by the electric current, clocks *controlled* by the electric current, and clocks *set to time* by it.

When we say that no system previous to that about to be described has succeeded, we do not by any means imply that no results more or less satisfactory were obtained, nor that they were in any instance total failures: *nothing is a failure* (certainly not in practical science) which helps to lead up to the desired end. By saying "they did not succeed," we simply state an undeniable fact, that no one of them obtained a footing of general public acceptance: circuits of the narrowest limits and clocks in the smallest numbers were the extent of these efforts. The Wheat-

stone, in London, perhaps went the greatest length, but the speaker questions whether 25 clocks were ever coupled up on one distributor, or to one motor even under it. We *believe* that some five or six clocks in one of our medical schools are still worked by this system from a local motor, but, when the Wheatstone Company sold off, the attempt to work the system publicly, ended. Wherein, we ask, lay the causes of this want of success? We believe that it must be traced principally to a mistaken desire to make the *clock subservient to the system*, instead of the *system* subservient *to the clock*,—they rather tried to make clocks to be kept right by the system than prepared a simple system to set the clocks right. What, then, are the criteria of a successful system of synchronizing, such as, whilst claiming the sanction of science, shall obtain the approbation of the public?

1st, and pre-eminently, the clocks must each have a vitality independent of the time signals or correcting currents. No man of business would tolerate that the going of his clock should be at any moment liable to sudden stoppage by some breakdown on a telegraph wire or pneumatic tube; each clock must be able, if temporarily cut off from its governor, to show something like true time when left to itself, until at least the breakdown gang can get to work, and couple it once more up to the controlling power.

2nd. The system must be capable of adaptation to existing clocks of the most divergent type. Fancy facing the Directorate of the Great Inter-Continental Railway Company, Limited, with the statement that if they wish to mend their time they must sell off their 500 clocks and adopt something new, and only better in promise. We must be able to take clocks just *as* they are and *where* they are, and, leaving them still to do what they already do well, *carry the hands round;* our place is only to see they do so in proper time.

3rd. The system must be able to correct the clock errors, *whether fast or slow*. The idea of persuading a clock to oblige the imperfections of your system by always keeping one fixed gaining rate of some small and arbitrary quantity, is so pre-eminently foolish, that it is only a wonder how any practical head could ever have wasted its thought over such a plan; for if our

friend the clock would only keep some such uniform small rate, it would not want synchronizing; a minute movement of the pendulum bob downwards would synchronize it for ever! But the trouble of clockmakers is that it is impossible to make the most expensive standard regulators perfectly steady to any absolute rate; the ordinary office dial or clock positively declines any such absolute regularity, even in its faults. It is a poor excuse to a business man that his clock has "gone wrong," "just because it took to *losing* a few seconds a day instead of *gaining* a few." The times are generally fast, the time often slow, and we must, to be successful, be able to set our clock right, *whether fast or slow.*

Other advantages may be gained, but no system which does not satisfy the above three criteria can by any possibility obtain any firm footing or extended application.

Having thus hurriedly cleared the way a little, I now bring before you more particularly the system under review to-night, and premise my description by asking my audience to particularly remember that I shall be obliged to use the words *mode* and *system* during the remainder of this lecture, each with a distinctively different meaning, and I think I shall justify the assertion that no previous efforts ever got beyond a *mode:* it never in any one instance developed into a *system.* This [showing a synchronizer] is my *mode* of synchronizing clocks: my *system* associates it with various instruments you see here on the table and elsewhere, which I shall presently more fully describe, and whose functions are to ensure a certain, true, and punctual delivery of time signals to our synchronizer. The *mode* must be *systematically* worked.

My *mode* of setting clocks to true time is by one of these instruments which I call a "synchronizer," and which forms an automatic finger and thumb to take hold of the hand at the right moment and put it, fast or slow, in its right place. It is, you see, but a simple little machine after all. The trite observation, not seldom made when it has been shown, has been, "*Is that all?*" Yes; an electro-magnet and keeper, a pair of levers, and a little finger and thumb is "all:" it is "all" that is wanted; more would be superfluous, less impossible. Simple alike in con-

struction and in action; nothing to be put in order, nothing to be put out of order, it needs only to be put where it can do its work, and then left to do it with as little interference as possible. (See Plate I., Figs. A, B, and C.)

Before, however, I explain more exactly *how* to fix it in its place, I must stay a few moments to ask you to note that the instrument before you, as a mode of synchronizing, exactly fulfils our stipulated criteria.

1st. By it, by a simple variation of size, and sometimes somewhat of form to suit the particular clock to be acted on, any number of clocks, few or many, of any varying size or kind, can be synchronized to any agreed standard time-keeper. Mantelpiece clocks, turret clocks, office dials of every conceivable size and kind, succeed one another upon our lines, totally irrespective of how they come, the size of the clock making no difference.

2nd. These clocks are kept to time whether having otherwise either a gaining or a losing rate, even when such a rate amounts to many minutes a day, having *two* pins, or thumb as well as forefinger. They stand guard on either side of true hourly time, ready, as they both simultaneously close, either to push back the hand if too fast, or push it forward if too slow.

3rd. No alteration or interference with the clock is needed. The synchronizer, being fixed as far towards the end of the minute hand as possible, is often at a considerable distance from the clock movement; it simply requires screwing in its place, and the slot for the passage of the pins cut in the dial. The cost of furnishing is thus minimised, no small consideration where a large number have to be operated on, but especially advantageous where they have to come into use in provincial towns and other places where skilled electrical labour cannot be had; and I claim for my synchronizer, as the first ever invented, containing *within itself* all its parts, and being, when not in actual momentary use, absolutely disconnected from the clock.

4th. Any failure in the standard clock to send out, or the various wires to transmit, the time signals, leaves the clock going in the ordinary way, to be set to time by the next completed current.

We now come to explain more fully the manner of affixing the synchronizer to the clock, and to call attention to a few points to be looked to in so doing. Without touching upon the special circumstances which rarely occur, only two questions arise: should it be fixed on the wood of the case or the back of the dial? and can I get sufficiently far out into the minute circle of the dial to be able to use the extreme point of the minute hand, or if not I must place a small block behind it for the pins to catch in setting the clock? The former plan is far preferable: it saves time, secures the longest possible leverage, and allows the two setting pins to be as long as possible, viz., only just free of the glass. The notch should be sufficiently wide to allow the pins certain and perfect freedom of action.

Beyond this there ought to be nothing to consider, but the generality of clocks, and notably office dials, are of most imperfect make, with faults which cannot be excused even when not intended for being synchronized. The first and principal one is the looseness of the minute hand on the square of the socket. It is evident that the slightest play here is greatly increased at the end of the hand: the whole of this play is of course so much lost accuracy in setting, and this fit of the hand must be made, not better, but *absolutely tight*. The hand should further run parallel with the dial, which it seldom does, generally owing to the pin which secures it being made much too taper, and so tight on one side and loose on the other; this pin should be as nearly straight as possible. The only other point is the minute spring, which is usually left thick and stubborn, instead of being thin, hard, and well "set up." If the clock, however common, has been originally made and finished with ordinary care, these points *should* require no attention; in point of fact, however, they generally do. When thus fixed the clock with its synchronizer appears as Plate I., Fig. D.

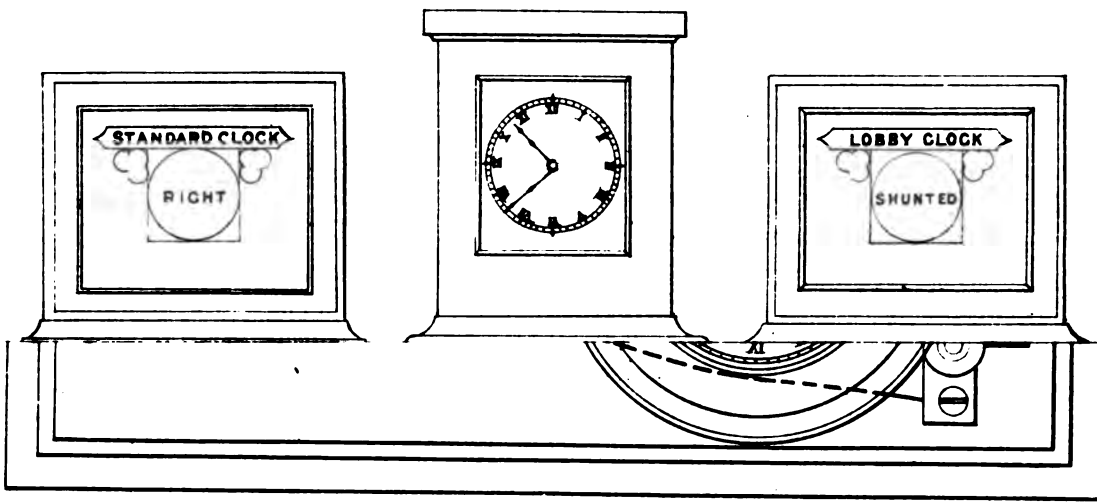
I have, I trust, thus made the explanation of our *mode* of synchronizing sufficiently plain, and I now go on to justify my assertion that a complete *system* needs to be arranged and developed if the *mode*, however perfect in itself, is to have a proper success; for it is evident that synchronizing is a matter involving a good deal more than a cursory consideration would lead one to suppose.

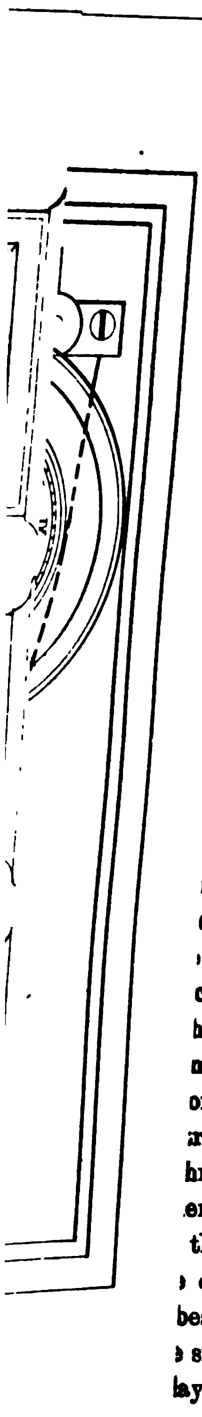
Thus, without a correct standard to punctually send out the needed time signals, and a well-erected and well-maintained system of telegraphic wires to transmit and deliver them, all our care and cleverness in synchronizing would be vitiated and nullified; and I have but little need to enlarge, to my present audience at least, on what is involved in such a system of telegraphic wires,—the perils of wind, weather, ignorance, mischief, and carelessness,—though I hope to show you to how great a degree the effects of these may be minimised.

To speak then, first, of our standards, of which, as I shall presently explain, we have two, they should be the best and simplest that science and experience can produce,—gravity escapements and such like toys for amateurs have no place in the piece of practical time-keeping we want. So we satisfy ourselves with Graham's dead-beat escapements, high numbered pinions, jewelled pallets, and careful finish.

I will now, as clearly and distinctly as I possibly can, give you a *detailed* description of the instruments involved in my system.

The most important of all must, of course, be our standard or controlling clock, of which we have two. Only one is now here; the other is a similar one in every way. The one is called the "Standard," in contradistinction to the second clock, which is called the "Lobby Clock." The necessity for adopting this second clock arose from the fact that some time ago, after the system had worked satisfactorily, I was horrified one Monday morning to find that, owing to the standard not having been wound, no signals were sent to the synchronizers. The possible repetition of this difficulty troubled me for some time, until the idea suggested itself to employ a second standard clock, wound up at a different time to the first, and so reduce the risk of both running down to a minimum. I still felt dissatisfied, and thought if I could get a system by which a breakdown should correct itself, the difficulty might then be fairly considered to have been got over. This is how the arrangement was effected. We have two clocks, which we name for distinction respectively "Standard" and "Lobby," and connected with them an arrangement which I call a "fault finder," and which, as you see, consists of three instruments, a


STANDARD CLOCK
RIGHT
LOBBY CLOCK
SHUNTED



ıepiece, and two indicators marking "Standard," Lobby," "shunted." As long as everything goes right t way, the indicators carrying the words "at work" " do not move. But should the "standard" clock fail to do its business, through having run down, contact, or any occurrence which results in a failure n of the signal, the hand of the small timepiece usual course, and presently the indicator drops and lard," "*missed.*" This movement is well so far as it oes more, it immediately shunts the whole system, by m clock No. 1, "Standard," to clock No. 2, "Lobby," sends out the next signal, and in doing so drops its Lobby," "*at work.*" Knowing the possibility of dust and stopping mechanism of this automatic nature, I nto the circuit a kind of "arch deceiver," a little which at eight o'clock each morning pretends a failure of the "Standard" clock, which immediately acts on clock No. 2 sends out the 9.0 a.m. signal, and leaves rs thus: "Standard," "missed,"—"Lobby," "at work." at the office each morning a glance shows that the ective measures are in working order, and two pull antly readjust the dial. (See Plate II.)

st perfect immunity from non-transmission of signals is ed even in the middle of the night, when no one is in to set in motion anything which had stopped.

come to describe an instrument of great value to a busy has to deal with a time-current system. It is this, that missing the opportunity of adjusting time when the om Greenwich is received (because otherwise engaged), roduced an "automatic error recorder," consisting of a hronograph with split hand and the ordinary inking ent, which makes a little dot wherever it points when the the pendant is pressed in. At a few minutes before ten dotting chronograph is set to the standard, absolutely beat. It is then placed in the recording instrument, and signal arrives at ten o'clock (which is the truest signal ay), it rings a little bell and makes a dot on the dial,

which dot, and not the hand, remains a permanent record of the error of the standard at ten o'clock a.m. (See Plate III.) Being now in possession of a "known error," the next step is to correct it, which in the ordinary way is by setting the hands, and, when necessary, a slight movement of the adjusting screw of the pendulum to make it go faster or slower as required. But a clock regulated in this manner is a source of constant irritation, as the slightest touch to the pendulum to arrest it, for instance, if a few tenths of a second fast, produces a new variation in its going, a trouble inherent in the very finest regulators. It was therefore found necessary to devise some means for setting a standard clock without what is termed any "physical" contact of any kind at all. The earliest attempts in this direction were by jumping small weights on to little *tables* on the sides of the pendulum. There is a very beautiful clock of this kind at Messrs. Usher & Coles', Clerkenwell, one of the prettiest mechanical "setters" I have seen. The weights are jumped on or off the table by triggers, and as soon as the requisite effect has been produced they are *jumped off again*. However, that plan had serious defects, and some one was required to be in attendance: sometimes the weights rebounded off the tables, and so on.

The credit of inventing a clock-setter absolutely without any physical or mechanical contact with the pendulum, lies with Sir George Airy. It consists of a permanent bar magnet fixed in a vertical position on the pendulum of the normal clock, and a coil of covered copper wire, without an iron core, known as a resistance coil, placed also in a vertical position immediately underneath the magnet, is fixed to the clock case. The coil occupies such a position that, when the pendulum hangs at rest, the end of the magnet is exactly over the centre of the coil. At every vibration of the pendulum the lower end of the magnet passes close over the upper end of the coil. The wires of the coil are led to a commutator placed in th[illegible]puting-room. By turning the handle of this commut[illegible]de, a battery current is placed in connection with t[illegible]rning it to the opposite side [illegible]tion of [illegible]nt is reversed. In one case [illegible]acent ends of the magnet and

which dot and not the [illegible] remains a permanent

coil; in the other case there is repulsion. When attraction exists, pendulum, being continually pulled towards the centre, is ed; when repulsion exists, the pendulum, being continually om the centre, is retarded. When the handle of the tor stands upright, the current is cut off from the coil, ffect is produced on the pendulum.

plan is a long way in advance of previous ones, completely any physical interference with the pendulum, but it still more attention than is compatible with my plan of verything as far automatically as possible, and I have for years used the following modification of Sir G. Airy's plan erfect success:—My instruments consist, besides the resist- oil and magnet, of a commutator marked "Standard" and oy," which by a plug operates either clock by one set of ments; a commutator marked "Fast" and "Slow," by the effect to be produced on the clock is determined; and ll lever timepiece requiring winding only once a year, by the exact length of time the current should remain on can orked off (Plate IV., Figs. 1, 2, 3, 4). By these means, one ation to the instruments is alone required, and as soon as the k is to true time, the current goes off, and no further effect be produced than was determined as needful.

We have now corrected our clock to Greenwich time, and are r prepared to distribute true time signals for this purpose. I e gone in a different direction to the chronofer used at the Post ice. My instrument is only five inches by two; a circular eel in shape, and I call it a "distributor." I divide London to twelve distinct circuits or districts. The signals sent over ese circuits (each circuit having its own battery and line) nounts to over 7,000,000 since the instrument before you was ced. It is something, I think, to be able to say for a little in- rument like that, that it distributes time from the extreme south f Kennington to the extreme north of Hoxton, and from the man- ions of the rich in the west to the docks in the east: the whole area (with one exception at the West-end) is worked from the little instrument before you. (See Plate IV., Fig. 5.)

We all know what "burning" points are, and I should like to

mention one matter in the hope that some of the scientific gentlemen present may be able to explain it. When the means of distributing at present used were first adopted, we were warned that the contact springs of our little distributor would burn, and very great care was taken in looking after the cleanness and brightness of those contacts; but through pressure of business this carefulness was relaxed, and nothing was done to the contact springs for some six months, and it was with great fear that I then looked to see what condition they were in. To my surprise I found that each contact had its volcano and crater [drawing sketch on board]. This was the result of the platina spring burning when contact was made with the gold screw underneath. The bottoms of the craters were perfectly bright, and, if large enough, I suppose you could have seen your face in them, and so I found that we had better leave the springs alone till they have burned a hole through. The contacts are found in practice to keep bright and clean so as to cause no hindrance at all to the transmission of the signal.

The PRESIDENT: What is the metal you use?

Mr. LUND: Platinum and gold. As to the lines, the only point of interest is that we are, as far as possible, endeavouring to work what are called metallic circuits. We start from one of the springs on the distributor, travel, say, down Goswell Road, back to the City Road, and return again to the distributor, making the complete circuit without a return earth current. The advantage of this arrangement is that in case of a thunderstorm there is no possibility of the lightning attacking the wire. Lightning but very rarely attacks a wire open at one end, whilst a wire open at both ends is perfectly safe. Such a wire gives further advantages in the way of testing, and in the economical use of battery power. We have not yet succeeded in supplying all London with loops of this kind, but have very nearly so.

We further utilise these metallic circuits thus: we have, say, a looped metallic circuit starting from Cornhill, going by the City Road to Old Street. The corner of Old Street is the most important centre in that neighbourhood, and from it we have erected branch lines, varying in direction and number, to supply what may be called the subordinate circuits, and it used to be

necessary to send once a week to see if these lines were in proper working order. To obviate this, and as I always prefer using automatic action, I was determined to see whether I could not make the branch lines automatically report their condition to the central office in Cornhill. I was told that such a thing was impossible, but I have nevertheless succeeded in obtaining the desired end. The arrangement is as follows:—

In this arrangement two clocks and an indicator are employed. The indicator and one clock are in the instrument-room, and the other one at the place where branches come into the main circuit. At a given moment the latter automatically breaks open the loop, takes the first half of the line, and connects it to branch line No. 1; at the same exact time the clock in the instrument-room connects the indicator No. 1 to a small battery, and, if all is in working order, the indicator flies open and announces the fact, and so on for the required number of branch lines. This arrangement is repeated twice a day, 9 a.m. and 9 p.m., to *prove* the working condition of the branch lines emanating from the main line. I think I am justified in saying that this is the first attempt made to automatically test branch lines at the middle of a main line, without any one in attendance.

(The 9 o'clock time signal has just gone through, and Mr. Webb reminds me that I was to show you how the several clocks you see here were adjusted in one circuit. I had all the clocks in the room set to different times in the same hour, and intended to draw your attention to the passing of the signal, but, unfortunately, the 8 o'clock current, which I had shunted off, was by some mistake shunted on again, and allowed to go through them, and consequently my arrangement was upset as the adjustment was prematurely brought about.)

I have shown you that we have a perfect standard clock, as near as possible a perfect distributor, and that we have automatical reports to say whether the wires are doing their duty or are broken down. We now come to the various modes adopted to utilise the signals when they arrive. As a general rule the signal is used for synchronizing (there are some 400 *synchronizers* in use in London), but in some few cases it is preferred to have a visual or oral signal.

The first one of the latter kind put up was in connection with the Stock Exchange. The clock there was synchronized with the idea of always using it for time bargains, but a dispute arose sometimes between members as to the fraction of a minute. A bell was proposed and fixed, but the hubbub usually prevalent on the Exchange prevented the bell being heard. An appeal was made to me, and I suggested a small time-ball to fall down as a standard, but I was warned that the members would inevitably use it as a target for missiles, and so that idea was abandoned. I then suggested a flashing signal, *i.e.*, a red disc fixed so as not to be visible in its normal position, but which should be brought into full view by the passing of the time signal, and remain so for three seconds. That plan answered its purpose perfectly, and for the past three years all time bargains on the Stock Exchange have been settled by it.

Time-balls I need not much comment upon: they have gone out of fashion very much on account of the trouble of pulling up the ball by hand, as is usually done, or the frequent winding up of the clockwork. However, to suit demands that arise, we have nearly perfected one which for one signal a day will need winding up only once a month, and with greater frequency according to the extra signals a day. It is arranged that as soon as the ball arrives at the top it immediately stops the clockwork, thus avoiding useless running. On arrival of the current the ball drops, sets the clockwork in motion, and, after being down five minutes, begins to ascend, and on arriving at the top of the standard the clockwork motion ceases till the arrival of the next current.

As to how far this system can be utilised,—of course in London the distances are comparatively short, ranging from a few yards to a mile or two, but to show to what extent it is available, I may mention that a clock has been synchronized at Dumbarton (a distance of 400 miles) for two years, without a single failure. I have recently received a most eulogistic letter from the town-clerk of that place, saying that everybody is delighted with the arrangement, which still works perfectly satisfactorily.

I was asked a little while ago to get a gentleman out of a dilemma. He said there was frequent trouble with his workmen

about time, and asked if I would synchronize his clock. I did so, but the men were not satisfied, and said the timekeeper did not always know when the signal went off. It then became necessary to adopt an automatic timekeeper, and this took the shape of a set of ordinary vibrating electric bells, connected with a ringing clock and a commutator *with three shunts in it*, marked "Sunday"—"Week-days"—"Saturday." These shunts were employed to switch off the current for such days as Sundays, Bank-holidays, or Saturday afternoons, when signals were not required. [The system was illustrated and explained by apparatus on the table.]

I have endeavoured to show you some of these matters in detail, and you will have perceived, as I said at the commencement, that there is nothing very new except the application of electricity to carry out some of the necessities of everyday life. It has been a matter of four years in working these things out. It of course seems very easy when it is done, but it has required a great deal of thought sometimes to discover how to get over these various difficulties, and I claim, not for myself personally, but for London, the city in which I live, and for England, my native country, that it has the largest and most perfect system of synchronized clocks in existence.

Sir Charles Bright: In expressing my thanks to Mr. Lund for his very excellent paper, I should like to refer to a system of time regulation in use many years ago, which appeared to me at the time (and I have no reason to change my opinion) to be the simplest method of regulating clocks by electricity. It was that of Mr. R. L. Jones, who in 1857 patented a system of regulating clocks which is no doubt known to Mr. Lund. It was applied at that time in Liverpool to some extent (to regulate some eight or ten clocks), and, I believe, is still at work in the Post Office there, if not for the same number, to some extent. It consisted of a standard clock, fixed at the Liverpool Observatory, under the charge of Mr. Hartnup, the astronomer there. Each movement of the standard clock sent a current of one kind or the other. The system of regulation which Mr. Lund has described is a periodical

adjustment, with long periods of non-adjustment, the currents being sent every hour, if I understood the description correctly—

Mr. LUND: Yes.

SIR CHARLES BRIGHT: And that a finger, or something equivalent to it, takes hold of the hand and moves it backwards or forwards as the case may be. The system of which I am speaking differed in this respect, that the regulation was continual. The power required for the regulation was very small, because it was only requisite to control a pendulum of a clock with ordinary works by currents sent from the standard clock each second. Each of the clocks thus regulated had a pendulum bob like that of the motor pendulum in a Bain clock, and a comparatively feeble current from one or two cells controlled, and I believe still regulates the clocks perfectly, advancing their speed or retarding it in unison with the standard clock at the Observatory. This clock controlled, among others, the Liverpool Town Hall clock (a ricketty old piece of mechanism, which used previously to stop sometimes, by the action of the wind upon the hands), a very good clock on the Exchange flags, and a large clock in the tower at the docks, besides several clocks intermediate on the circuit. That system continued to work exceedingly well during the 12 or 13 years that I had to do with it, and I believe has continued to do so under the charge of the Government. The system has the advantage of great simplicity. You have only to take any magnetic pendulum bob and two small magnets, and you have the whole apparatus necessary. I cannot think of any more simple method, and certainly none would work better as a fixed regulator from which any one could adjust his watch or clock, at any moment in the hour, to a second; and I think myself, subject to correction from other people's experience, that that is the best form of regulation if you are going for a true system of regulation. Of course, if it is desired to use a wire for other purposes, and to have an occasional setting of hands (which I do not consider a complete system) during the intervals between which people may lose a train, for example, then perfection must give way to economy.

The PRESIDENT: I shall be glad to hear any further observa-

tions on Mr. Lund's very interesting communication. He has pointed out many practical difficulties which have occurred in developing his system of regulating clocks, and the way in which they have been overcome. We have many members present who are competent to criticise Mr. Lund's arrangements, and to point out defects, if any still remain. As Mr. Lund has said, the merit of such a system must depend upon the way in which the practical details are carried out. There is no matter of principle involved, but the difficulty in such a case is to apply known principles in such a way as to produce the effects, and that apparently Mr. Lund has done with very great success.

Mr. A. STROH: I have listened with great pleasure to Mr. Lund's interesting paper, and I perfectly agree with him that clocks ought simply to be controlled by the electric current. One great advantage, which Mr. Lund did not mention, of this system seems to be, that the line wires employed in it can be used for telegraphic messages, or anything else, when not in use for the controlling of the clocks, which takes only a very short space of time indeed in every hour. A system of the kind spoken of by Sir C. Bright has not this advantage, because the wire is employed continuously. I should like to ask Mr. Lund one question. These clocks may go fast or slow, and are set right every hour. Has anything been done to endeavour to shorten or lengthen the pendulum at the same time when the setting of the hands takes place? If the time current could be made to adjust the pendulum as well as the hands, the clocks would eventually become so regulated that they would neither gain nor lose, and the occurrence of a breakdown in the transmission of the signal would be of little moment.

Mr. WALTER H. COFFIN, F.C.S.: I trust, Mr. Chairman, though not yet an elected member, I may be allowed to make a remark called forth by Mr. Stroh's enquiries, without anticipating Mr. Lund's reply. Some years ago, before I was aware of Mr. Lund's system of synchronizing clocks, I was connected with an attempt in America to do a similar thing. We attempted both to set the hands to correct time and regulate the length of the pendulum. We accomplished the setting of the hands by a method similar to Mr. Lund's, but perhaps more simple. We also had an arrange-

ment by which, when the hand was behind time, the electrical current was sent in one direction and shortened the bob; if the hand was ahead of time, the current went in the other direction and the bob of the pendulum was lengthened; and thus any error accummulating by difference of temperature was continually neutralised, and also the regulation was proportional to the error of the clock. If the clock was very slow, the pendulum would be shortened more; if, on the other hand, the error were very small, the correction would be very small. It was so arranged that if the error was within a certain limit there would be no correction whatever, but the whole was merely experimental and finally abandoned. I merely mention it because it was interesting from a scientific point of view, and might perhaps overcome some objections in Mr. Lund's system. One seems to me that if a clock is continually going slower or faster, or a correction or two is missed, a time will come when the hand will be in such a position that one of the little "fingers," instead of regulating, will be jammed at the top of the hand, and the clock will be stopped. Whereas, if the possible error could be kept within certain limits, by correcting the pendulum as Mr. Stroh suggests, it could never happen that the hand would be jammed by the apparatus, and the accidental omission or suspension of the current would be less serious. I must apologise for the time I have occupied in making these remarks.

Mr. G. S. Criswick, F.R.A.S.: I should like to make a remark in reference to what Mr. Lund said as to the contact springs in his distributor. At Greenwich we did not find the difficulty anticipated by Mr. Lund—want of contact; but, on the contrary, the fusing of the metal made a continuous connection, and a constant current was set up which to us was ruinous. The difference between Mr. Lund's experience and ours may be that Mr. Lund's springs are stiffer than ours. That difficulty has been got rid of to a great extent by introducing a resistance coil of about six times that of the circuit, which seems to kill the spark and enables us to get on comparatively comfortably. The frequent sticking of the springs caused by fusion of the points of contact was the great difficulty we had to contend with.

The President: The coil is inserted as a shunt to the circuit?

Mr. Criswick: Yes; the constant current not only caused a failure of the chronographic register, but always disturbed the rate of the clock.

Mr. J. A. Lund: Before replying, Mr. President, I would apologise for having omitted to give an explanation of this large circular instrument before you. It is what we call a fault-finder. It consists of a number of drop indicators, corresponding with the number of springs on the distributor. The instrument achieves the curious result of making a broken line report its own break-down. Two chattering bells (tuned to a fifth) are joined up in circuit, and as long as the signals pass through and all goes well, the bells are silent, but should a breakdown occur on one of the wires, five minutes after it happens both the bells ring violently until attention is given, and owing to this speedy announcement of failure it seldom happens that we have breakdowns lasting more than two hours.

[The detail connections of the instrument were described on the board.]

I have not much to say in reply to what has been said, but I will take up one point. It has been suggested that the clock should also be regulated if possible. Now, those who know what a system of synchronizing is, will, I think, upon consideration, say at once that it is practically impossible to do so, for this reason, that you must in that case have pendulums of one length, or at all events of two lengths, half-seconds, and seconds. You cannot connect up a dining-room clock, an American dial, a large dial clock, and so on, with each other: it is impossible to send a current to affect the pendulums of all three in equal time and in equal proportion. We have ourselves made a great many experiments in this direction, and have, I think, hit upon something (which, perhaps, on a future occasion I may have the opportunity of laying before the Society) by which we shall be able to check a clock, but the experiments have not so far been such that enable me to explain to you at present.

There was one very important remark made by Mr. Stroh with reference to a point which I purposely left out to see whether it was noticed—I mean with reference to using time-current wires.

for telegraph or telephone purposes. Experiments in this direction have been made, through the kindness of the United Telephone Company, between our establishment, 41, Cornhill, that Company's offices at Coleman Street, City, Broad Street, and Victoria, upon a circuit on which clocks were synchronized and telephonic communication carried on with perfect success.

I have a telephone in this room [turning to it].

The President (speaking through telephone): Who are you, and where are you?

Answer: Barraud & Lund's agents; office, Pall Mall.

Mr. Lund: The circuit extends from Pall Mall to this room, with telephones at each end, and *ten* clocks in circuit in between, at various points along the line.

The only remaining remark worth detaining you for is with reference to Sir C. Bright's observations about Mr. Jones's system. I think I was correct when I said at the beginning (and I most heartily say it) that every one that helps forward any result of this kind ought to be recognised. If a man makes an attempt to do a certain thing and miserably fails, he has not *utterly* failed, for the reason that he has enabled some one after him to avoid making the same mistake. And therefore I say that those who have laboured before myself deserve all credit for their patient perseverance, and perhaps more than myself, because they have been willing very often to take failure and still plough on, endeavouring to succeed. But, as an answer to Sir C. Bright, I would say that Mr. De la Rue, who for a long time was known as the most determined adherent of a slightly modified Jones's system used on a private wire between Greenwich and Bunhill Row, sent for me about 18 months ago, saying that he had tried the system in every way, but that it was a total failure, and he instructed us to remove it and replace it by our system. I did so, and it gives every satisfaction. Continuous control currents cannot be used on account of the varying lengths of the pendulums, and I therefore still adhere to my opinion, that we have followed the best plan to allow the clock to jog along (I do not care what kind of clock it is) for an hour, and if at the end of the hour it has not done quite what it ought to do, we set it right and let it go on again. If it

gains we put it back, and if it loses we put it forward, and so we are enabled to keep correct time at all places where the synchronisers are fixed.

Sir C. T. Bright: I call that setting a clock, not regulating it.

A hearty vote of thanks was accorded to Mr. Lund for his paper, which, Mr. Lund said, went a long way towards repaying him for his labours in producing the results he had shown.

A ballot for new members then took place, when the following gentlemen were elected; and the meeting adjourned until Thursday, November 24th, when a report on the International Exhibition of Electricity, by Sir C. T. Bright and Professor D. E. Hughes, F.R.S., was announced to be read :—

As Foreign Members:

Don Enrique de Arantave.	M. L. Hellings.
William D. Baldwin.	Captain Jöhnke.
Don José Battle.	F. W. Jones.
Louis Marcelin Borgon.	E. Mercadier.
Don Saturnino Islas y Bustamente.	Don Cristobal Ortiz.
Silvanus Gotendorf.	Don Engenio Fortun y Varona.
A. van Hasselt.	Philip Walker.

As Members:

Thomas Cushing, F.R.A.S.	John Hopkinson, M.A., D.Sc., F.R.S.
Professor George Forbes, F.R.S.E.	James Nelson Shoolbred, B.A., M. Inst. C.E.
James E. H. Gordon, B.A.	
Killingworth Hedges.	

As Associates:

Lieut. Ralph W. Anstruther, R.E.	Ernest George Hebbert.
Evelyn D. D. Berrington.	H. L. Hitchcock.
Walter H. Coffin, F.C.S.	William Leon Madgen.
Joseph G. S. Cunnington.	J. Kenneth D. Mackenzie.
Francis E. Gripper.	Alexander Charles Moffatt.
W. Tighe Hamilton.	Mark Ruddle.
Lieut. Christopher T. Hawker, R.E.	Colonel J. G. Sandeman.
	William Oliver Smith.

The Hundred and Fourth Ordinary General Meeting of the Society was held on Thursday evening, November 24th, 1881, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Professor G. C. FOSTER, F.R.S., President, in the Chair.

The minutes of the last meeting having been read and confirmed,

The SECRETARY announced that the following gentlemen had been transferred by the Council from the class of Associates to that of Members :—

Lieutenant PHILIP CARDEW, R.E.
R. E. CROMPTON.
J. FLETCHER MOULTON, F.R.S.

The SECRETARY then read the following paper :—

REPORT UPON THE INTERNATIONAL EXHIBITION OF ELECTRICITY IN PARIS, 1881.

By Sir CHARLES BRIGHT, M.I.C.E., and Professor HUGHES, F.R.S. (Members).

The important part taken by this Society in regard to the British section of the International Exhibition of Electricity at Paris, appears to call for some record in the Journal of the Society; and as many of our members were unable to see that unprecedented collection of industrial applications, now being dispersed to all parts of the world, we have (with the approval of our colleagues on the Council) drawn up a statement of the circumstances attending the Society's connection with it, and a description of some of the more prominent machinery and apparatus.

The Exhibition was first authorised to be held in the Palais de l'Industrie, and its organisation established by a decree of M. Grévy, the President of the French Republic, in October, 1880, and application was made to H.M. Government to appoint a commission to represent British interests there; but after some time had elapsed, our Government signified that it was not their intention to make such an appointment. Upon this being notified to M.

Berger, the Commissioner-General in Paris, he wrote (on the 5th March, 1881) to the Society, inviting its co-operation, and two days later, at a General Meeting of the Council, a committee was formed to represent the Society in all matters connected with the Exhibition, and to take all necessary steps to make the British section as complete and as successful as possible. Under their auspices, and with the untiring assistance of our Secretary, Mr. Webb, and of Mr. Aylmer, our honorary secretary in Paris—both of whom were subsequently appointed hon. secretaries to the Royal Commission—together with the energy and great enterprise of the exhibitors, the British section became worthy of our position in electrical progress, as testified by the awards of diplomas and medals apportioned by the juries at a later period. Still it must be admitted that, had more time been at our disposal, many departments of the science would have been more fully represented.

While alluding to the subject of awards, we feel some pride in stating that the one single name occurring in the official list of awards relating to this country, under the head of Diplomas of Honour to Ministers, Administrations, and Learned Societies, is that of the "Society of Telegraph Engineers and of Electricians."

At the latter end of June, the Government officially appointed a Commission to represent them, consisting of the Earl of Crawford and Balcarres, Lieut.-Col. Webber, R.E., and the writers of this report, by whom the work relating to the British section of the Exhibition was carried on until its close on Sunday last, and we feel confident that the work has been done to the satisfaction of the exhibitors and the interest of the country.

Referring now to the salient features of the Exhibition, we must premise that it is impossible, within the limits of a paper to be read at one of our evening meetings, to do more than mention in a condensed form some of the most striking or more novel exhibits.

Our object will be easiest attained by conceiving that we are conducting a visitor through the building.

The Palais de l'Industrie has a grand nave 600 feet in length, and about half that in breadth; it has also a series of galleries and rooms on the upper story. The south side of the nave was

given up almost entirely to the dynamo-electric machines, the steam and gas engines (giving a total of 1,800 horse-power), the boilers, and the counter-shafting which were needed for generating the powerful currents of electricity required all over the building. The main part of this nave was divided into two equal parts, one of which was devoted to the French nation, and the other to foreign countries. The galleries and rooms on the upper story were used for miscellaneous exhibits, but were also largely utilised for illustrating the applicability of the electric light to domestic purposes. The main entrance was in the middle of the north side, and the first object of attraction was the lighting of that part by the Siemens and Werdeman lamps. The former were very efficient: each group of lamps was fed by the current from a separate machine. The latter were exceedingly steady, and gave much satisfaction for this reason, although expensive to work.

The staircases to the right and left of the entrance were lighted by six Pilsen lamps, so called, not from the names of the inventors (Messrs. Piesse and Krizik), but after the village in Bohemia of that name. We found this lamp from repeated observation to be very satisfactory in its working.

On entering the nave from the vestibule there was, in front of the visitor, a large and handsomely constructed lighthouse, supplied with a Fresnel's lens and an electric light, worked by the powerful current derived from a De Meritens machine. A preference has been given to this magneto-electric machine for lighthouse purposes by the French and English Governments, and, of course, by employing permanent magnets, no part of the current is used up to maintain the magnetic field; but the great value of such a machine is that the magnetic field is absolutely constant in intensity, however much the steam-engine or other power employed may vary in speed; and there can be no doubt that steadiness (which is a great factor in lighthouse work) is very much increased by the use of permanent magnets.

A miniature lake surrounded the lighthouse, and it was here that M. Trouvé exhibited his boat, worked by his electro-motor and a bichromate battery, the motor being attached to the fore-

part of the rudder, and not to the stern-post. We did not find that it realised a practical speed, but the space for its operation was too limited for any useful trial.

In front of the lake there was a collection of Serrin lamps, especially constructed for use in lighthouses and on board ship. To test their capabilities under this last head, they were mounted in such a manner as to demonstrate that they could be twisted into all possible positions, or set into oscillation without affecting the steadiness of the light.

On turning to the left from entering the nave, we came first to the British section, of which a catalogue upon an enlarged scale was published in a special number of the Journal of this Society.

The first, and perhaps the principal object attracting attention, was the pavilion of our Postal Telegraph Department, which not only contained the latest forms of Sir Charles Wheatstone's automatic and other instruments, but also a most comprehensive collection of great historical value to any one who might be engaged in writing an account of the early growth of telegraphs. Here were the earliest five-needle telegraphs of Cooke and Wheatstone, of 1837, the first telegraph practically at work. Here, also, were many exhibits of scientific interest, amongst which may be mentioned Professor Hughes' induction balance. This apparatus—well known, of course, to the members of the Society—had several interesting practical applications during the Exhibition, amongst which may be cited the case of one of our distinguished foreign members, Mr. Elisha Gray, who said to Professor Hughes: "Some thirty years ago a scrap of iron entered my finger while at work: it got deeper the more I tried to get it out, and I left it alone: try if your balance will find it." On trial none of Mr. Gray's fingers disturbed the balance, except the one containing the piece of metal, which did so unmistakably when placed in the coil.

The department of electrical science in which the British section was in advance of all other nations was that of Submarine Telegraphy.

The Submarine Telegraph Company showed a piece of the first experimental cable laid in 1850 between Dover and Calais, and specimens of all their cables, late and early. The Telegraph

Construction and Maintenance Company, and the India Rubber, Gutta Percha, and Telegraph Works Company (known more to most of us as the Silvertown Company), both the English main works and the French branch at Persan-Beaumont, made a good show in the same direction.

Messrs. Latimer Clark, Muirhead, & Co. showed cables covered with their new insulating material called "nigrite," which promises to be much cheaper than the compositions now in use, but it will have to pass through the ordeal of time, as gutta percha has done before it, prior to being adopted by engineers for lines of great length. It is made of the black wax left as a residue or by product in paraffin distillation, mixed with india-rubber. Mr. Muirhead's duplex working for long cables, which has been used with such advantage on the Eastern Telegraph Company's system, was also exhibited. The last-named company had Sir William Thomson's syphon recorder actually in work through an artificial cable of 1,200 miles.

In the same section Messrs. Siemens Brothers had erected an imposing trophy consisting of a telegraph buoy, surrounded at its base by nearly all the most interesting objects associated with submarine telegraphy, including a model of their telegraph ship the "Faraday," which has done such good service in cable-laying.

In connection with cables, we ought to mention the Brooks subterranean cable in the Silvertown exhibit. It consists of an iron pipe containing a large number of conducting wires covered with jute to prevent contact between the wires, and the insulation is given to it by liquid paraffin always kept under pressure by a column of the same liquid in stand-pipes (which can be replenished from time to time if and when required), so that any leak will be outwards and not inwards.

Messrs. Elliott Brothers had an excellent collection of their telegraph and testing instruments, so also had Messrs. Clark & Muirhead. Muirhead's compound iron and steel posts have the advantage of combining strength with lightness, so that in a country difficult of access considerable economy is secured.

The great dimensions of Mr. Spottiswoode's induction coil, made by Apps, attracted considerable attention, and experiments

were occasionally made with it. We believe that in England we have the largest induction coil, the largest electro-magnet (Lord Crawford's), and a battery consisting of the greatest number of elements (Dr. De la Rue's), but it was only the first of these that was shown.

Near to this, the fire-alarm system of Mr. Edward Bright was illustrated by eight street posts, similar to those fixed by the Fire Brigade in London, for the use of the police or the public in case of fire. On pulling out a handle an extra resistance is thrown into the circuit, passing from the central station through all the posts, the amount of the resistance differing at each post. This disturbs a balance of resistance at the central station and rings a bell, when the fireman on watch turns a handle, which inserts resistances in the circuit corresponding to those in the posts, so that when the bell stops ringing the handle points to the place whence the alarm proceeds, and this without clockwork or anything that can suffer from exposure to air or moisture in the posts.

Professors Ayrton and Perry showed some useful and very ingenious apparatus for use in electric light measurements. To measure the motive power used in a machine, they have a shaft-coupling in which two discs are connected by springs, so that there is a relative twist, the amount of which depends upon the force used. This is magnified by levers, and read off in a way depending upon the persistance of visual impressions, the horse-power being obtained by multiplying by the speed of revolutions.

In front of the office of the British Commission, Mr. Crompton had a collection of lamps, and the several parts of a dynamo-machine made upon the plan of M. Burgin, the workmanship of both lamp and machine being exceedingly praiseworthy.

On the same table Mr. Spagnoletti showed his system of fire-alarms, in which a ball runs down inclined planes, and in doing so makes a series of electrical contacts which move a step by step instrument at the central station. He also showed his system of railway signal locking levers. A signalman cannot let a train pass along a line until he has received permission from the man at the other end of the section, and this man cannot give such permission until the last train to which he gave leave has actually passed his station.

Messrs. Saxby & Farmer also showed their system of railway signalling in action on the full scale.

Near this place we noticed a small but interesting collection of Mr. Conrad Cooke, the principal object in which was a galvanic cell and galvanometer combined, in which spiral glass tubes composing part of the cell surrounded an astatic pair of needles. A telephone call and some historical instruments were also of much interest, but we are obliged to condense our description so much, that in this, as in other cases, we have no choice but to pass on.

Messrs. Siemens Brothers showed a large number of dynamo-machines of different sizes, besides specimens of their pendulum and differential lamps. They had also some induction machines arranged both for quantity and intensity, for exploding mines and torpedoes. Among their testing instruments were a mirror-galvanometer-scale of ground glass, with a long mirror above the scale, and a long mirror below the needle. The advantages of this arrangement are that the readings can be made with far greater ease and accuracy. Their electro-dynamometer, in which the torsion required to bring the instrument to zero is measured, is a convenient appliance for use with alternate currents.

The newest piece of apparatus shown by them was the electrical furnace, in which the most refractory metals can be melted in the arc formed by two carbons of large section, or by a group of carbons. We were fortunate enough to witness several highly successful experiments on the melting of steel, and it would be interesting to know how far the properties of the steel are altered by this treatment.

The firm of Siemens & Halske, in the German section, was close to the last-mentioned exhibit. It showed a very complete and most interesting history of the continual progress of the firm in the construction of telegraphs, dynamo machines, electric lamps, galvanoplasty, and numerous applications of electricity to the arts of war and peace. Not the least important was the collection of very fine testing apparatus which was displayed in their office.

The German section was generally of very high interest, many scientific men, such as Wiedemann, Kohlrausch, and others, having

sent the apparatus employed in their well-known researches. We were also interested to see exact copies (in one case we believe we should say the original) of the early experimental telegraphs of Soemmering, of Steinheil, and of Gauss and Weber. The German Post Office authorities also sent a fine collection. Heilmann-Ducommun and Steinlen had a complete workshop fitted up with lathes, planing machines, and drills driven by Gramme dynamo machines, all of them of unusual excellence of manufacture. The North German Refining Society of Hamburg exhibited specimens of pure metals deposited by six Gramme machines, producing at their works 550 tons of pure copper annually. They have also a new process by which they succeed in extracting pure gold from its alloys, and have by this method prepared 2,700 pounds weight of gold during the year 1880.

Returning now to the office of the British Commission, and continuing along the passage, we should arrive at the Austrian section. One of the most important exhibits was a simple form of the Pilsen lamp, which we have already mentioned. The type here shown was of an horizontal form, and worked exceedingly well. There was also an interesting telephone by Machalski, in which the transmitter is a tube of powdered charcoal, and the receiver a Bell telephone of the Siemens type. The chief feature of this combination is that the voice can be heard at a considerable distance from the receiver. A very fine exhibit was also shown by Schäffler of his well-known telegraphic apparatus, meteorological registering appliances, and electrical clocks.

In the Norwegian section, close by, the object of the greatest attraction was the series of experiments by Dr. Bjerknes, in which he finds complete analogies to magnetical and electrical phenomena by setting up vibrations in diaphragms immersed in a trough of water. Two cylinders are employed, with pistons worked by a revolving wheel, so that the air is alternately compressed and rarefied, and india-rubber tubes can be connected either to the front or back part of the cylinders, so that vibrations can be set up through these air tubes, either synchronous or in opposition to each other. The india-rubber tubing is connected to the various instruments by which he puts the water into vibration. Our time would

again not permit an apparatus of this kind, which deserves more notice, to be fully described, but we shall probably have the opportunity of seeing it at one of our meetings hereafter.

The next section in the order which we are taking was the Russian, where there were worthy of notice some early experimental apparatus by Jacobi and others, some Siemens machines attached directly to rotary engines, and some excellent galvanoplastic reproductions of works of art.

The Italian Government exhibited their interesting collection of instruments in a building in the style of the Doge's palace in Venice. The most attractive object, on account of claims which have been put forward on behalf of Signor Pacinotti, since dynamo machines have been developed, was a small machine or model of the ring with teeth between its coils, designed as an electro-motor in 1860, but described as being capable of being used as a magneto-electric machine in the *Nuovo Cimento* in 1864, a copy of the drawing in which was kindly given by the Italian Commissioner to any person making application for it. The apparatus as there shown would not have made a dynamo machine equalling those we now possess, but, considering the principles involved, the model had for us a very great interest. The other Italian exhibits of an historical kind were in the upper story, and will be alluded to hereafter.

In the contiguous Danish exhibit was a new form of Gramme machine by Professor Jürgensen, comprising a fixed electro-magnet in the interior of the ring to increase the induction. The mechanical construction of the machine is difficult, but theoretically the advantages seem deserving of being followed up.

In the neighbouring Swedish section the wonderful meteorological self-registering apparatus of M. Theorell was constantly at work. It gives the reading of the instruments every quarter of an hour, printed in numerals, and in a tabular form. The hour, the velocity of the wind, its direction (in numbers from 1 to 32), the temperature, the humidity, and the barometric reading are all printed in bold figures.

The Belgian Observatory showed an apparatus for a similar purpose by Professor Rysselberghe, but in this machine, which

records its observations every ten minutes, the results are not printed in type, but are engraved on a metal drum as curves, which can be printed off on to paper directly.

The Belgians showed many things of great interest. First, there is the Jaspar lamp, which appeared to us to be remarkable for its regularity of action. Then there was the arrangement of double-conducting wires in telephones by M. Brasseur, for getting rid of induction by twisting two wires in opposite directions round each telephone, and leading one end of each wire to earth; thus the currents in the line wires travel in opposite directions, and an induced current will produce two contrary effects in the telephone, but the resistance, instead of being double, as in the ordinary method of employing two wires, is actually halved, or nearly so, as in actual practice. This method is identical with that first described by Professor Hughes in his paper upon "Induction," read before this Society, March 12th, 1879.

The "Lampe Soleil" was also shown here, and has attracted some attention, but its efficiency is not very high. The electric control system of clocks in Brussels, extending over the entire city, was also shown, and attracted much attention.

Next to the Eelgian section came the American, where Mr. Elisha Gray's harmonic telegraph (now working, as we are informed, on one wire, with six instruments, between New York and Boston) was shown in action. The principles of its operation have been described in a paper read before our Society several years since, so that we need not allude to it further, except to draw attention to the circumstance of its being now at work upon the multiple system.

There was also a useful little motor, consisting of a Gramme ring, or tube inside another tube, so wound as to give the Gramme a suitable magnetic field. It was worked by bichromate batteries, and was used with good effect for driving sewing machines, of which we may say, while speaking of them, that there were great numbers in the building, driven by M. Marcel Deprez' and other electric motors.

The American exhibits also comprised an apparatus for telephone exchange subscribers' work to be done at the central office,

which differs in no important respect from switches of the same character used in Europe.

The Dolbear telephone possesses some special interest, because it differs essentially from the Bell or electro-magnetic telephones belonging to the class now known as the Condenser Telephone. The transmitter is an ordinary microphonic sender and an induction coil of high resistance is employed; the line wire is connected at the receiving end to a thin disc of ferrotype iron, and a similar disc is placed immediately opposite to it, very close, but insulated from it. This latter disc may be connected to a return wire or to the earth. Variations in the primary current produce static charges of opposite electricities in the two discs; they attract each other with a force depending upon the charge, and articulate speech can thus be successfully transmitted, and, in fact, has been communicated for as great a distance as from London to Norwich.

In the Swiss section there were shown the excellent systems of electric clocks by M. Hipp, and also an extremely ingenious apparatus by Professor Monnier for automatically analysing the fire-damp in a mine, and transmitting the information to the pit-mouth every hour. Twelve of them being placed in different parts of the mine can be connected with a single receiver, each one being connected to it for five minutes in each hour, this being the time required for the analysis to be completed.

We have now passed in review most of the objects of special note in the foreign side of the main body of the nave, and are now at the east end of the building, whence the electrical tramcar of Messrs. Siemens Brothers leaves the building to go to the Place de la Concorde. The current is generated by a powerful dynamo machine within the building; it is then carried by two metallic tubes split on the under side, and mounted on poles like telegraph wires; a carrier, attached by two wires to the car, consisting of two contact-pieces, runs inside of the two split tubes, and thus the current is conveyed to the car. Here the current passing through a dynamo machine sets it into rotation, and by a chain connection to the wheels of the tramcar, their velocity being reduced by this gearing to suit the speed required.

The east end of the building under the gallery was devoted, so

far as lighting was concerned, to the Brush Company, who showed a very large exhibit consisting of six Robey engines, and a large number of machines from a small size up to those by which forty arc lights were fed. A large number of Lane Fox lamps were exhibited by the same company. Passing now down the passage at the south side were to be seen in succession all the dynamo machines at work, some driven by steam-engines and some by gas-engines. Here was Thomson & Sterne's latest form of gas-engine, and beside it Brotherhood's adaptation of his three-cylinder engine to Gramme, Siemens, and Brush machines. Behind them was the Dowson gas-generator for heating purposes, for which great economy is claimed. By means of the gas itself some water is heated, and the steam with air is injected into a furnace which decomposes it, anthracite coal being employed, liberating carbonic oxide and hydrogen, which pass through a purifier and thence to the gas-holder. Messrs. Rowatt & Fyfe showed some Pilsen and Joel lamps, and also a Schuckert's machine. Schuckert, Gülcher, and Naglo each had a Gramme machine with a flat ring, with the magnets at the side of the ring. So also the White House Mills (United States) and Horne (Berlin) had each a Siemens alternate machine without any deviation in principle. The Weston and Maxim machines were like a Siemens armature with Gramme field-magnets, and *vice versâ*. The former has the strips of the commutator inclined, and not parallel to the axis.

The British Electric Light Company had some large-sized Gramme machines of new pattern, and well-established engine power, which they applied to Brockie lamps, and to their own incandescent lamps. Crompton employed a machine made by Burgin to light his lamps, which were suspended high up from the top of the dome.

Swan's lamps, of which about 1,000 were in use, were fed by Brush and Siemens alternate current machines.

Passing to the machinery in the French section, we noticed some very large gas-engines, some of 40 horse-power, and some very powerful and highly-finished steam engines. They were mostly employed to drive the machines of Gramme, De Meritens, and Lontin. M. Gramme showed a large number of experimental

machines which he has tried at different times, and many of which he has abandoned.

The principal lamps in this section were the new Gramme lamp, the Jablochkoff, and the Jamin. Messrs. Sauter & Lemonnier exhibited Colonel Mangin's reflectors for military purposes, which were in some cases a mètre in diameter; also the dynamometer of M. Mègy, which indicates directly on an engine counter the amount of work which has been exerted on a machine in any given time.

At the end of the passage was the exhibit of the Force et Lumière Company, where their secondary batteries were employed in lighting incandescent lamps and in driving motors, mostly of small size.

The most interesting as well as the largest exhibit of the whole of the Exposition was that of the French Minister of Posts and Telegraphs, who had a large pavilion filled, not only with specimens at work of all the forms of apparatus used by the French Government, but also many of historical interest, and some which have hardly yet passed out of the experimental stage. The radiophonic experiments of M. Mercadier were also shown here, and employés were in attendance to explain the action of the instruments and show them actually at work.

The different railway companies had large space allotted to them for exhibiting full-sized specimens of their signalling and other electrical contrivances. The Chemin de Fer de l'Est had a very completely equipped dynamo-metric waggon, for indicating at any moment the speed, the traction, the pressure of the brakes, etc.

The Ministers of War and of the Marine showed field electric light apparatus, chronographs, and complete ballistic instruments for studying pressures and the velocities of projectiles. M. Christofle exhibited a fine collection of galvanoplastic works of art, and several processes, some of them quite new, actually at work. M. Planté's collection of secondary batteries and their applications, to which he has devoted so many years of study, was also to be seen here.

In the French part of the nave there were also numerous applications of the transmission of electricity to produce motive power in sewing machines, in embroidering, in mining, in pump-

ing large volumes of water, in cutting stone, in ploughing, and in workshop machinery. In the exhibit of Messrs. Siemens Frères, there were also shown small electrical carriages, weighing a few pounds, intended to replace the pneumatic despatch.

Ascending now to the upper story, the first striking exhibit consisted of a suite of large rooms specially illuminated to illustrate the feasibility of domestic lighting. A picture gallery was illuminated by the "Lampe Soleil," a theatre by the Werdeman system, salons by the Reynier and Jamin lamps; a kitchen, a billiard-room, a bath-room, and a dining-room, all fitted up, were lighted by separate systems. Here also were the telephone rooms connected with the opera, which were visited by thousands of people every evening on which there was a performance. Fourteen microphones were placed before the footlights of the opera connected with telephones in these rooms, besides others in the private room of the Minister of Posts and Telegraphs. The telephones used were Ader's modification of the Gower-Bell telephone, and it may be fairly said that the effects exceeded the most sanguine expectations.

Some of the upper rooms were lighted by the Swan, Edison, and Maxim lights, and others by the arc lights of Siemens, but of these the most pleasing effect was produced by M. Jaspar's method of throwing the light upwards upon white screens while concealing the direct light from view.

Two of the upper rooms were filled with Mr. Edison's exhibits of the phonograph, telephones, and incandescent lamps, which were (during the greater time that the Exhibition was open) used with a current derived from some machines of moderate size. At the end of October a large machine was installed, the armature of which weighed 3½ tons; the poles of the field magnets of this machine are magnetised by five electro-magnets for the upper, and three for the lower one. The principle upon which the armature is constructed is the same as that in a Siemens machine. The axis is connected directly to the steam-engine, and turns at the rate of 350 revolutions per minute. The machine is designed to feed 1,000 incandescent lamps, and is said to consume 120 horse-power.

One of the upper rooms was devoted to historical apparatus, and here the Royal Institution showed a great many of the original instruments used by Faraday in his experimental researches. Ampère's electro-dynamic apparatus was also there, and the Italians showed a great number of interesting apparatus and documents, photographs of letters from Galvani and Volta, apparatus used by Galileo, by Volta, by Marianini (his pupil), by Nobili, by Zamboni and by Melloni. Never has such a collection been brought together under one roof. King's College also contributed a most interesting collection of Wheatstone's original experimental apparatus, and some of Daniell's.

In concluding this general summary of this remarkable Exhibition, we are bound to confess that it has been impossible for us in so short a compass to do complete justice either to the Exhibition as a whole, or it may be to individual exhibitors; but our object has been to give some idea to those of our members who had not the opportunity of being present, of the magnitude of the exhibition and the direction in which it has proved that our science has been advancing.

We cannot conclude our rapid glance at the Exhibition itself without mentioning the Electrical Congress, held in the Congress Room in the same building.

The Congress consisted of some two hundred of the most distinguished savants of Europe, all nominated by their respective Governments. The Congress was not open to the public, and the press was not admitted, consequently until the official publication of the proceedings, which will shortly take place, we are not justified in giving any account of these remarkable meetings. It may, however, be said that the Congress was a most brilliant success, and that we may soon hope to have a paper before the Society giving a résumé of its discussions and decisions.

The visit of our Society, with the whole of its official staff and many of its members, was also an interesting event, as well as a merited compliment to the grandeur and brilliancy of the Exposition.

The electrical world owes its thanks to Monsieur Cochery, the Minister of Posts and Telegraphs, and to Monsieur Georges Berger

the Commissaire General of the Exhibition, not only for the energy and judgment which made the Exhibition such a perfect success, but also for the uniform kindness and attention shown by them and their whole staff to the foreign Commissioners and exhibitors. We all equally laboured to ensure success, and we feel sure that our British exhibitors, as well as those of all nations, have reason to be proud of the great success of this the first International Exhibition of Electricity.

The PRESIDENT: The paper has not dealt with matters which can very well admit of dispute, but I hope that we may have some interesting conversation, if not exactly discussion, on the subject. Many members present were at the Exhibition, and the report teems with suggestions and matters which invite interesting remarks.

Professor W. E. AYRTON: I am sure we must all be delighted to hear the admirable résumé which the two authors of the paper have given us of what was exhibited at the Paris Exhibition; and, as they have given us so much information, perhaps it is rather unfair to ask them if they would not mind giving us a little more about one point which I think many like myself would be interested in, viz., what sort of experiments were the jury able to make in trying the efficiency of the vast collection of apparatus for electric lighting that was shown in the Exhibition? We know that there was a large staff and competent jury appointed, and it would be most interesting, in connection with the labours of the jury that may have to carry out similar duties at the Crystal Palace Exhibition, to hear some details of the methods that were employed at Paris, and to learn what success the labours of the jury met with.

Sir C. T. BRIGHT: By one of the regulations (which I think was a very proper one), neither any of the Commissioners nor any of the exhibitors—such as Professors Ayrton and Perry were, with their show of very excellent appliances—should, according to our rules, be on the jury, and of course the experiments as well as the decisions of the jury were kept entirely to themselves. I see one or two faces around me who were on the jury: Mr. Stroh, who is

a member of our Council; Mr. Moulton, another member of our Council; and Professor George Forbes, were all on the jury, and are present, and can, I am sure, supply the information to Professor Ayrton better than myself, because I did not attend any of their deliberations.

Mr. J. F. MOULTON: The English Courts of Law have one extremely good custom, that no juryman can ever be put into the box and asked what he decided or why he did it; and among the jury at the Paris Exhibition, though there was the wildest variety of opinions on almost every possible subject, there was absolute unanimity on the advisability of holding our tongues. However, as I fortunately was not on that part of the jury that had to deal with the matters that would interest this Society most, *i.e.*, the part which had to do with the modern developments in the way of lighting and dynamo machines, I think that I shall probably gain by setting the example of breaking this rule to a certain extent, for I shall involve my friend Professor Forbes in the necessity of telling the Society something which will be very interesting to them and to me. He can tell us all about the exhibition of electric lighting; for not only did he go through all the labour of examining the lamps, but there was no member of that jury who did his work in a more conscientious and complete manner.

For my own part, I belonged to that section of the jury that had to examine the instruments for electric measurements, such as galvanometers and electrometers, etc., and I think I can say that the general feeling of the jury that had to do with that class was that the makers of those instruments were waiting for the world to come up to them; for the exquisite delicacy of the instruments, and their accuracy, was such that neither science in its practical nor in its theoretical form—by which I mean the science of the workshop or the science of the laboratory—required any greater perfection. I am glad to say that the English showed well in this class. In the list which was drawn up in order of merit, and on which the medals were given, there were three English names in the first eight, and those three occupied by no means the lowest places. But although the English did so admirably, and

proved by their exhibition that they were not behind in the race, I think that those who went to the Paris Exhibition, and who were capable of judging of such instruments, must have agreed with the jury in their high opinion of the marvellous exhibition of Dr. Werner Siemens, of Berlin, in this particular class. I do not know when or where it would be possible to see such an admirable collection of every form of instrument designed for electric measurement. Not only did they possess all appliances for accuracy, but many of the varied properties of electricity and the galvanic current were most skilfully made to assist in rendering the instruments more useful to the observer. The degree of accuracy with which these instruments were capable of doing their duty was something almost incredible, and I hardly know how many ciphers would come before the first significant figure in representing the decimal of an ohm which some of them could measure. But in giving high praise to the exhibits, I am not simply referring to these instruments of Dr. Werner Siemens, but also to those of what I may call the first exhibitors in the class. Not only were the instruments extremely accurate, but they were extremely practical: it did not seem as if delicacy in the sense of sensitiveness had produced delicacy in the sense of over-delicacy or of unpracticality in them; and I am only speaking the feeling of my fellow-jurymen when I say that we were thoroughly pleased with the whole exhibit of the class, and that we thought that in it electricity was certainly doing all that could be demanded of it.

Our jury had another small class under their care, *i.e.*, the class of static electricity; but I am sorry to say that in that subject the world is as nearly as possible where it was many years ago. Scarcely any advance has been made, a fact which shows that, after all, the true stimulus to advance in this world is practical utility, and that those branches of electricity which are not in the purview of the man who has to deal with practical science are likely to be much slower in their progress than others—a conclusion which cannot but be agreeable to a Society like our own, which, without undervaluing theory, takes practical utility as one of its main aims.

Professor G. FORBES: I certainly did not know that I was

going to be called on to speak this evening: I came to listen. In the first place I must compliment Mr. Moulton on the excellent way in which he has got out of it as a juryman. I have a sort of feeling that gentlemen who have been members on a jury ought not to be called on at a later period to explain the reasons and so forth of their decisions, because each one tried no doubt to do his best; and it seems hard lines that each one of us ought to have to explain the exact reasons why different awards have been given. I feel this all the more because Mr. Moulton has particularly drawn attention to me as an electric light juryman, that is to say, one who had to decide on the merits of the different electric light systems.

I am afraid that although we made the most careful measurements, experiments, and tests (such as I think Professor Ayrton would be content with), at the same time, owing perhaps to some political movement which caused the Ministry to go out a little earlier than intended, the awards had to be given more quickly than was compatible with the requirements of the jury themselves; so that, in obedience, it may be, to the wishes of the Minister of Posts and Telegraphs, we perhaps were sometimes obliged to comply with what has been described as Jedburgh justice. In other words, we decided the relative merits as far as our stern decision could possibly go, and then, after having done so, we applied the most splendid tests that could be applied to those lamps and machines to which the awards had already been given. That is to say, that although we did our utmost to distinguish between the merits of those lamps and machines which we perfectly knew to be nearly on a par, yet those lamps and those machines were tested again, and many of them before the awards were actually given. Most of the dynamical tests were conducted by M. Tresca, who had already had great experience in that line of research, assisted by a most competent committee; and if the results be published, I am perfectly certain that they will be about the most valuable results which have ever been issued in connection with electric lighting up to the present time.

The experiments which were made were of two classes. One with a fixed set of apparatus, with steam-engine, dynamometers,

and so forth on a fixed place, with a machine set running, placed at our disposal, and photometric appliances in a dark room separately. In this instance the power which was consumed, as indicated by the steam-engine, was registered at the same time that measures were being made as to the candle-power of the lights. The other system—one on which I insisted very strongly, because it enabled us to measure a large number of different systems which we could not have measured otherwise (and which we were able to measure before the jury awards were announced) —was simply to put indicators on to the different steam-engines which were in use. The machines were planted in the best possible way, presumably, in which the exhibitors could show them, and indicators were put upon them and diagrams taken; and at the same time photometric measurements were made on the lamps, careful account being taken of the number of lamps in circuit at the same time, and of the regularity of the lamps. Thus the two systems were, first, to employ the steam-engines and machines just as they were fitted up by the exhibitors, and register the horse-power employed per lamp as indicated by the steam-engine, which is a very important thing; and, secondly, to put the dynamo machines on to the steam-engine which was specially provided for M. Tresca, and to examine the horse-power that was employed in each of the different systems.

No doubt the great point to be arrived at is the horse-power consumed per candle-power, and I do not know that the system employed at Paris was the best, but I believe that it was as efficient as anything could have been under the circumstances. At the beginning of the jury trials, I certainly confess that I feared that sufficient attention might not be paid to the subject, but at the end of it I am convinced that a most thorough system of trials was undertaken; and the only thing I fear is that it may not be possible to publish all the results (as several of the most important members of the jury said), because it might compromise the exhibitors. If they are published, I am certain that they will be the most important results which have ever been published in connection with the subject of electric lighting.

The PRESIDENT: I am sure that the Society will forgive

Professor Ayrton's indiscretion (if it was one) in inviting the members of the jury to violate the secrecy which they had vowed to each other. We have had from both Mr. Moulton and Professor Forbes extremely interesting remarks, and I think it is matter for great satisfaction that it should be publicly known that the awards of the jury, which will be shortly announced, have not depended upon general impressions formed by any persons, however eminent, but have been the results of deliberate and careful examination and testing of the objects to which they relate.

Professor C. A. ABEL: As regards the exhibits relating to the application of electricity to naval and military purposes, I regret that I cannot bring before the Society any remarks which would be of special interest to them. There were some interesting exhibits of the kind, but I think, with perhaps one exception, they were already well known, not merely to this Society, but to those gentlemen in this country generally who have devoted their attention to the subject. The applications of electricity, for example, to mining purposes, to military telegraph purposes, and so on, have been before us in various ways on several occasions.

The only exhibit of special interest in the first-named direction which I noticed as a novelty was a small instrument constructed by Burgin (on the same principle as his large dynamo machine for the production of the electric light), which appeared to be (though I had no means of testing on the spot) a very efficient dynamo machine for military purposes. There seemed to be advantages connected with it in reference especially to the accumulation of the maximum force of the machine before it was shunted into the exploding circuit. Among the equipments exhibited in connection with military telegraphy, that in the American section much struck those who examined them, mainly in regard to their simplicity and apparent efficiency, especially perhaps in respect to portions not actually electrical, namely, those relating to transport of the equipments. Some duplex instruments without condensers, adapted for military use, exhibited in the Belgian and Spanish sections, attracted the attention of military men; but beyond the matters I have mentioned, and some ingenious appliances connected with ballistic science,

I do not think there was anything connected with military matters of special interest. Our military-telegraphic and mining department certainly showed conspicuously by the complete absence of exhibits, and probably to this is to be ascribed the (I will not say want of knowledge, but) want of information exhibited by some correspondents of important papers in this country, who sent us from time to time most interesting descriptions of the various branches of the Exhibition, but who told us some things that made those who were acquainted with the condition of military telegraphy in this country smile at the apparently benighted condition in which we were supposed to be in connection with this particular branch of the application of electricity. I may tell the members of this Society that in this branch we had nothing important to learn at the Exhibition; and if our Government had thought fit to allow those who are connected with this branch of the service to contribute an exhibition of military-telegraphic and other appliances, England would certainly have stood pre-eminent in this direction. I therefore can only say that I trust, when there is another electrical exhibition, official routine and official reserve will not prevent our standing in the front rank in this particular branch, as we should do.

Mr. E. Graves: I have only to express the great satisfaction with which I have heard the very lucid, clear, and explanatory report on the facts of the Exhibition that has been compiled by Sir C. T. Bright and Professor D. E. Hughes. The Post Office exhibit was, as has been stated, a collection of apparatus intended mainly to show the historical development of telegraphy in England, from the commencement of its practical operation until the present day. There were some modern instruments, no doubt, but they were exhibited rather as the sequence of the foregoing: they were not displayed in action, and there was no attempt to bring them into competitive comparison, so to speak, with those of foreign nations. It was the desire of the English Government to avoid anything beyond a complimentary display that should be creditable to England *as a display*, but should at the same time avoid anything like interference with the *amour propre* of any foreign nation. Our position was rather that of self-abnegation than of

attempting to claim for ourselves any prominent place. That we succeeded in the object that we proposed to ourselves, I think I may fairly claim, though we were pretty well abused by a good many newspapers because we did not do more; nevertheless, as we intended to do no more than we did, I do not think I need apologise for the shortcoming.

The PRESIDENT: We have been hearing about the past Exhibition, which has been a great success; perhaps we may have the pleasure of hearing something about a future Exhibition, which we hope will be a similar success. Major S. Flood Page is, I believe, present with us, and perhaps he can give us an indication of the arrangements made for the forthcoming Exhibition at the Crystal Palace.

Major S. FLOOD PAGE: Mr. Chairman and gentlemen,—We certainly move very quickly in these days, but I must confess that I was hardly prepared that we should pass so rapidly from the immediate past into the very near future. I cannot help feeling, Sir, that, speaking for the moment on behalf of the Crystal Palace, and so far as the Exhibition which we are going to have at the Crystal Palace may be considered a national affair, so far on behalf of the nation, that we may be very much encouraged by the report that we have just heard. Nearly every English exhibit mentioned in that report as being of the first interest will also be exhibited at the Crystal Palace; and we are very much encouraged, Sir, by the fact that so very large a number of the distinguished members of your Council have honoured us by assisting us with their knowledge and advice in the arrangements in progress for our Exhibition, and have become members of our Honorary Council of Advice. In my opinion, and time will show whether I am correct, we have been able to learn something from the marvellous Exhibition that has just been held in Paris—I mean in the way of a more scientific arrangement of the electric light. All who went to Paris must have felt, especially on the ground floor, the impossibility of judging the effect of the various systems. Of course, as Professor Forbes has told us, the jury in Paris gave their decision from a scientific point of view, and it is a matter (as you yourself have said, Sir) of the greatest possible satisfaction to know that

they devoted themselves thoroughly to their work, and based their decisions upon scientific grounds. But there is another point of view in considering an exhibition of the electric light, viz., there are many men connected with railways, with large works, corporations, local boards, and others, who wish to have the satisfaction of judging for themselves with reference to the effect of different lights. It is our intention to divide the Palace for electric lighting in such a manner that no single system will interfere with any other system, except so far as it may borrow laterally as one gas light does from another, and every one will be able to judge of the effect of each system by itself.

There is only one system of importance of electric lighting at Paris which will not be represented at the Crystal Palace, that is the system of Mr. Jaspar, of Liége, who led me to believe he would come over, but for some reason or other he has not yet sent in any application, while several systems will be seen for the first time in public. We shall have one advantage over the Paris Exhibition, viz., that it will be in the winter, when the Palace can be lighted up at four or five o'clock in the afternoon, a more convenient hour for visitors than that necessary (eight o'clock) at Paris. I cannot conclude those few impromptu remarks without alluding (in the presence of Mr. Graves) with gratitude to the fact that Her Majesty's Post Office have been good enough to undertake to send, and will send, a larger and more important exhibit than they had at Paris. And (in the presence of Professor Abel) I can only go on to say that we have asked the War Office and Admiralty to do the same thing; and if Professor Abel will use his influence, and can kindly induce the War Office to send the things familiar to him, but not to the British public, we shall be extremely glad. Just one other word. We are going to pass a vote of thanks to Sir C. T. Bright and Professor Hughes for their paper, but we must not forget the other British Commissioners, and we can hardly mention them without a feeling of shame coming over us as Englishmen, and at the same time, as Englishmen, a feeling of pride—shame, when we remember that our Government was so shabby as not to pay a single penny of the expenses of the Commissioners; and pride, when we realise that these gentlemen gave

months of their valuable time, and hundreds of pounds out of their pockets, in order that English electrical science should be worthily represented in Paris at the first International Electric Exhibition.

Mr. A. STROH, Member: One interesting subject which came to my notice during the Exposition in Paris was the manufacture of permanent magnets. Having myself been engaged in the manufacture of a great many permanent magnets, I know the difficulties which exist in obtaining the proper quality of steel for that purpose. Now, in the Paris Exhibition the permanent magnets were specially under my notice as a juror, and I found that most of the makers of permanent magnets who exhibited used one particular kind of steel, a kind of cast-steel manufactured by Messrs. Charrière et Cie., Allevard (Isère), France. *It is* called "acier d'Allevard." This steel appears to be well known in France, and the exhibitors showed great willingness in stating that they make use of it as the best kind of steel for magnets; but it never came under my notice in England, and I believe that, if it was only known to English manufacturers of permanent magnets, they would get good assistance by its use. For instance, in the Gower-Bell telephones we have, some time ago in this room, admired the great power of the magnet. M. Trouvé had in the Exhibition magnets which carried 45 to 50 times their own weight. The magnets in the Alliance machine, and in that of M. de Meritens, for producing electric light, and many other powerful magnets, appear to be all made of this steel. The method of hardening and magnetising differs with each manufacturer, but I know from my own experience that, having once obtained the right steel, the principal difficulty is overcome, and it does not matter much which process is employed for hardening and magnetising.

Mr. A. LE NEVE FOSTER: May I be permitted to say a few words on the subject of permanent magnets? I have been listening to the remarks Mr. Stroh has just made with regard to the use in France of Allevard steel, and can endorse a great deal that he has said. Like Mr. Stroh, I have had some considerable experience in the manufacture of magnets, and, further, have for this purpose for

the last eighteen months or so been using large quantities of the Allevard steel. I have made a number of experiments with various qualities of steel, and perhaps the results obtained may not be without interest to the members of this Society.

Of each particular class of steel tried, quantitive analyses have been made, and as an invariable rule I have found that there is a very marked superiority in all the samples of steel which contain in their composition tungsten. The Allevard steel proves on analysis to contain some three per cent. of tungsten, and it is to this fact that I attribute in a great measure its superior magnetic qualities. With a sample of German steel containing about half this quantity of tungsten, I have had very fair results; whilst with a number of other samples of steel said to contain tungsten, I have been surprised at the bad results, until, on analysis in each case, it was proved that no tungsten existed, and, as probably many of you are aware that tungsten has very marked reactions, even a very small percentage would be detected. It appears to me, as far as I have gone at present, that the presence of tungsten in steel very materially improves its magnetic qualities, but how far this holds good, and what part other substances may play in the composition of steel, I am unable to say.

I have been assured as a fact that the manufacturers of the Allevard steel were not themselves aware only very recently to what to attribute the superiority of their magnet steel, so that with them its manufacture was a matter of chance, not being acquainted with its peculiar composition. I am not certain, but believe I have heard that Dr. Siemens mentioned at a meeting of the Steel Institute, that he had found the presence of tungsten in steel to improve its magnetic qualities; at any rate, from my own experiments with magnets, I certainly have arrived at this conclusion.

Sir C. T. Bright: Professor Hughes and myself are very much pleased if we have given any information about the Exhibition to any of those who had not the opportunity of going over to Paris. We do not seem to have had any difference of opinion during our discussion, and I do not know that the paper itself was likely to give rise to any. There is one thing I may add, though

I have not the exact figures, but there was a very considerable profit, something like £17,000, accrued, as the result of the Exhibition, to the French Government. The total power of the electric light exhibited must have been more than a million candles, and the total horse-power of the engines employed to drive the dynamo machines, and for other purposes, was upwards of 1,800 horse-power. I had hoped to be able to announce the total number of visitors to the Exhibition, but I received a reply from Paris this morning to the effect that the total was not yet made out.

Mr. R. E. Crompton: Before we separate this evening, I think that we who were Exhibitors at Paris should testify to the very great services we received from the Commissioners, and from the Secretary to the British section of the Exhibition. Those who were present in Paris when the Exhibition was incomplete, in its early stage, when the whole of the space set apart for the dynamo machines was rough like a ploughed field, will call to mind how they saw our Commissioners, hard-working scientific men of the eminence of Lord Crawford, Professor Hughes, and others, hurrying about in their shirt sleeves, in fact, doing our work for us, and assisting us in every way to get our exhibits into order. I believe but few of you are aware of this. I myself feel how much I, as one of these exhibitors, am indebted to the Commissioners for their aid and assistance. We should never have been ready in time but for them, and I believe most of the other exhibitors were similarly situated. One gentleman's name should not be passed over this evening: it is that of the Secretary, Mr. J. Aylmer. (Hear, hear.) He worked throughout the Exhibition in a perfectly untiring manner. He was always to be found at his post, from the first moment the Exhibition opened in the morning, and was the last man out at night. Such devotion to his duties must have been at the expense of his own private business, and I hope that this Society will, at some future meeting, take steps to co-operate with the rest of the English exhibitors in showing our appreciation of Mr. Aylmer's services by some more substantial token than a mere vote of thanks.

The President: It now only remains for me to ask you to

pass a formal vote of thanks to Sir C. T. Bright and Professor Hughes, not only for the paper which we have had to-night, but for their great services, which Mr. Crompton has reminded us of, as Commissioners for the English portion of the Exhibition.

Mr. J. F. MOULTON: I have great pleasure in seconding that resolution. Not being an exhibitor, and not having been in Paris at the commencement of the Exhibition, it is not for me to speak from personal experience of the most important part of the services that were rendered by the Commissioners, and Mr. Aylmer, their Secretary, but I have never seen men more thoroughly devoted to their work; and I think that the fact that the English exhibit was so satisfactory to all the visitors at the Exhibition was very largely due to their untiring energy, and to the way in which they absolutely devoted themselves all day and every day to their duties. For this paper and for such services as those, a vote of thanks is a very slight return, and I most heartily second the vote of thanks to Sir C. T. Bright and Professor Hughes.

The vote was expressed most heartily.

The meeting was then adjourned until the 15th December, when the Annual General Meeting will be held.

The Hundred and Fifth Ordinary General and Tenth Annual Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 15th, 1881—Professor G. C. FOSTER, F.R.S., President, in the Chair.

The minutes of the last meeting were read and confirmed, and the names of new candidates announced and suspended.

Voting papers for the election of officers for the ensuing year were deposited, and Mr. F. C. Webb and Mr. W. Ladd appointed scrutineers.

The SECRETARY then read the following reports:—

ANNUAL REPORT OF THE COUNCIL.

The Council have satisfaction in reporting that the additions made to the several classes of Members during the year are as follows:—

Honorary Members	1
Members	24
Associates	50
Students	1

In addition to which, 23 Candidates are announced for ballot this evening.

The losses by death, resignation, and other causes have amounted to 41, viz:—

Foreign Members	3
Members	11
Associates	27

Among the deaths we have to lament the loss of no less than five officers of the Royal Engineers, viz.—Col. Glover, Lieut.-Col. Anderson, C.M.G., Major Eckford, Capt. Heneage, and Lieut. Macgregor.

The Institution of Civil Engineers have continued, with their accustomed liberality, to allow the General Meetings of the Society to be held in their Lecture Hall.

The following Papers have been read during the Session:—

Some Experiments on Induction with the Telephone. By A. W. HEAVISIDE, Member.

Earth Currents—Electric Tides. By ALEX. J. S. ADAMS, Associate.

On the Application of Dynamo-Electric Machines to Railway Rolling Stock. By Lieut. P. CARDEW, R.E.

On the Interference with the Processes of Manufacture of Wool and Hair, arising from the development of Electricity during Spinning, with a description of Apparatus applied to obviate it. By EDWARD B. BRIGHT, Member.

Telegraphs in Japan. By J. MORRIS, Member, Local Honorary Secretary and Treasurer for Japan.

On the Application of Electricity to Lighting and Heating for Domestic and other Purposes. By ST. GEORGE LANE FOX PITT, Member.

On Recent Researches in Radiophony. By W. H. PREECE, F.R.S., Past President.

On the Construction and Working of a Military Field Telegraph (based upon experience gained during the Campaign in Afghanistan in 1878, 1879, 1880.) By P. V. Luke, Member.

A Résumé of the Earlier days of Electric Telegraphy. By WILLOUGHBY SMITH, Member.

"Radiophonie." By M. MERCADIER.

On Telegraphic Photography. By SHELFORD BIDWELL, M.A., Associate.

Les Chemins de Fer Electriques. By Dr. WERNER SIEMENS, Foreign Member, Honorary Secretary and Treasurer for Germany.

L'Anneau de M. Pacinotti. By M. le Professeur GOVI.

The System of Synchronizing Clocks adopted in London and elsewhere, with an exhibit of a complete set of all the various instruments connected with its working. By JOHN ALEXANDER LUND, Member.

Report upon the International Exhibition of Electricity in Paris, 1881. By Sir CHARLES BRIGHT, Member, and Professor D. E. HUGHES, F.R.S., Member.

On a Convenient Form of Constant Battery of very Small Resistance. By C. F. VARLEY, F.R.S., Member.

Among these, the communications of Mr. Adams, Mr. Preece, and Mr. Luke led to important discussions, while the communications of M. Mercadier and Professor Govi, read at the Paris meetings, were of unusual scientific interest.

Professor Helmholtz's visit to England in April last enabled the Society to evince their appreciation of that distinguished physicist's labours, by inviting him to meet its members and a large number of scientific men at a Conversazione in the Flaxman

Gallery and the library and adjoining rooms of the University College, which, through the good offices of the President, were kindly placed at the disposal of the Society for that purpose, by the Council of the College. Thanks to the readiness with which their efforts were seconded by many members of the Society, the Committee appointed to carry out the arrangements for the Conversazione were able, notwithstanding the very short time at their disposal, to bring together for exhibition during the evening a large collection of objects of great scientific and technical interest.

The year now drawing to a close has been made memorable in the annals of electrical science by the International Exhibition of Electricity which was opened in Paris in August last, and by the Congress of Electricians which met there about the same time, and continued its deliberations until October.

Both the Exhibition and the Congress owe their inception to M. Cochery, the French Minister of Posts and Telegraphs; and, looking to the undoubted impetus which the former has given to every branch of applied electricity, and the important questions which it is hoped will be settled by the latter, the Council believe that they have but given expression to the unanimous feeling of the members of the Society by electing M. Cochery an Honorary Member of the Society.

Her Majesty's Government having in the first instance declined to appoint Commissioners to the Exhibition, the Society were requested by the French Government to endeavour to bring the proposed Exhibition to the knowledge of English electricians, and to take measures to secure the due representation of Great Britain at that international competition.

Although so late in the field, the Committee which was immediately appointed by the Council, in response to the application of M. Berger, the Commissaire Général, succeeded in obtaining the co-operation of all the principal English companies and firms established to carry out the various branches of electrical engineering work, as well as the aid of most of the leading electricians. They were thus enabled to secure for the British Section of the Exhibition a display which, although perhaps not

so complete a representation of the position occupied by this country in relation to the science and practical applications of electricity as it might have been had longer time been available for organisation, was still not unworthy of the occasion. The energetic co-operation of our Secretary, Mr. F. H. Webb, contributed not a little to the successful result of the exertions of the Committee.

Some time before the Exhibition was opened, the Government, on the joint application of this Society and the Society of Arts, consented to appoint Commissioners, viz., The Earl of Crawford and Balcarres, F.R.S., Colonel C. E. Webber, R.E., Sir Chas. Bright, and Professor D. E. Hughes, F.R.S., through whose indefatigable exertions, ably seconded by our Local Honorary Secretary, Mr. J. Aylmer, the work commenced by the Society was most successfully carried on up to the last.

For the convenience of visitors to the Exhibition, the Council published a special catalogue of the British exhibits, containing a fuller description than was to be found in the French official catalogue.

The efforts of the Society in these matters were referred to in warm terms both by the Minister and the Commissaire Général upon more than one occasion, and have, moreover, been signally recognised by the distinguished compliment of a *diplôme d'honneur* being awarded to the Society by the jurors.

The assembly of so large a number of eminent electricians of all countries, among whom the Society could reckon many of its foreign members, suggested to your Council the desirability of holding a short séance of the Society in Paris, and accordingly two meetings were, by the kind consent of the Minister, held in the Salle des Conférences, in the Exhibition Building, on the 22nd and 24th of September. The meetings were attended by the Minister and by nearly all the eminent scientific men then in Paris, besides a large number of our home members, and the proceedings were rendered of great interest through the valuable communications on telegraphic and electrical science which were read upon those occasions.

The financial position of the Society is satisfactory: the income

has increased, and all our liabilities have been paid off, and it will now be in the power of the Council to commence the investment of the Life Compositions, which hitherto has been impracticable.

As will be seen by the report of the Librarian appended hereto, the Library has, by his prompt applications, received numerous accessions through the liberality of authors, while other additions have been made by purchase.

In accordance with the expressed assent of a very large majority of members at home and abroad, and in conformity with the resolution passed at the Special General Meeting held last December, the Society has adopted from the commencement of the year its amended title, by the addition of the words, "and of Electricians;" and the proportion of scientific names to be found among the list of members elected this year seems to afford evidence that this alteration of the title has certainly assisted in making better known a fact which appears to have been previously only partially recognised, viz., that the Society is established to promote Electrical Science in *all* its branches.

The proposed incorporation of the Society under the "Companies Act" has rendered it desirable that the Rules and Regulations should undergo some revision, and this matter has been under the careful consideration of a Special Committee appointed for that purpose. Their recommendations, approved by the Council, are now under the consideration of your Honorary Solicitor, with a view to framing the Articles of Association, which will shortly be submitted for your approval at a Special General Meeting.

The Council have much pleasure in recording the continued kind services of the several Local Honorary Secretaries, to whose exertions are due to a great extent the increase in the number of members elected during the year, and the reduction of the arrears of subscriptions, which at one time seriously crippled the action of the Society.

In conclusion, the Council feel justified in congratulating the members generally upon the firm position which the Society has attained in the scientific world.

SOCIETY OF TELEGRAPH ENGINEERS AND OF ELECTRICIANS,
4, THE SANCTUARY, LONDON, S.W.,
December 15*th*, 1881.

DEAR SIR,

I have the honour to submit, for the information of the Council, the following Report upon the Library of the Society for the present year.

The Library was declared open to the Members on the 10th November, 1880, and, on the 1st January this year, was thrown open with certain restrictions, to the public generally. In order to meet the convenience of those not able to visit the Library in the daytime, it was decided to have the Library open in the evening till 8 o'clock on four evenings in each week, and this privilege has been taken advantage of by many Members of the Society and others.

The number of visitors since the 1st January this year has been as follows:—

Members	180
Non-Members	158
Total ...	338

The visitors were not very numerous during the early part of the year, owing no doubt to the fact that the opening of the Library was not generally known, nor was the attendance during the summer months very great; but from the number of visitors during the past two months, I venture to think that now the Library is becoming better known, its usefulness will be more appreciated, and the attendance greater.

I would again draw attention to the valuable collection of Specifications of Electrical Patents which the Society possesses. These specifications are presented by H.M. Commissioners of Patents, on application being made for them; and I have made it my especial duty to compile the lists each week in order that the specifications may be obtained immediately after publication. Visitors to the Library are therefore able to refer to all the latest published specifications on electricity, magnetism, and their applica-

tion, without the necessity of a previous search, and a journey to the Patent Office for the purpose. The time and trouble thus saved must therefore in many cases be considerable, and the fact that the Society possesses such a collection cannot be too widely known. I am glad to say that this collection has already been found of great value, and has been much referred to. I may add that there have been more than 430 electrical patents taken out since January 1st this year. While referring to electrical patents, I would mention that the Official Gazette of the United States Patent Office, containing short abstracts, with illustrations, of all the United States Patents, is received in the Library every week; a complete set from January 1st having been presented.

The large number of English and foreign periodicals which are received have been as far as possible completed and bound up, and they form one of the most valuable portions of the Library.

At the end of this report I have appended a catalogue of the accessions to the Library during the year. It will be seen that these accessions are very numerous, more than 165* books and papers having been acquired, many of them of great value and importance. The greater portion of these have been presented by the authors, and the thanks of the Society are due to those gentlemen who have, either voluntarily or at my request, furnished the Library with copies of their works.

I desire particularly to draw attention to a collection of documents presented by the Bureau International des Administrations Télégraphiques, Berne. These documents form, together with those which were previously in the Library, a complete collection (with one exception) of the works published by the Bureau. The Society is to be congratulated upon having received this valuable present.

My visit to Paris enabled me to obtain several works both by presentation and purchase, some of which I should not otherwise have heard of, and which have been accordingly added to the Library. Through the kindness of Prof. Hughes and Mr. Aylmer,

* These figures do not include the works presented by Mr. G. J. Symons, F.R.S., hereafter referred to.

I obtained special permission to examine several of the very valuable and interesting collections of books exhibited at the Paris Exhibition; and it is satisfactory to know that, with the exception of Mr. Latimer Clark's collection, they contained but few rare books which the Society itself could not have exhibited.

One of the important results of the Paris Exhibition has been tho publication of an Italian bibliography of Electricity and Magnetism, which is stated, in the preface to the work, to have been partly compiled from the Ronalds Catalogue.

I have, during the past two years, compiled a classified subject index to the Journal of the Society, from the beginning to the present time, which I forward herewith, and which I shall be happy to place at the disposal of the Society. Should the Council accept it, I will make it complete up to the end of the tenth volume, the last number of which will be published at the end of the year. The compilation of this index has been a work of great labour, and was undertaken to supply a want which has been felt for a long time past, and the M.S. has already been found of much value.

The limited sum placed at the disposal of the Library Committee for the purchase of new books will not permit very much to be done towards perfecting the Library, but I shall always keep this object in view, and from time to time, where possible, obtain such works as are required to make the Library as complete as its importance deserves.

I am, dear Sir,

Yours faithfully,

A. J. FROST,

Librarian.

F. H. Webb, Esq.,

Secretary of The Society of

Telegraph Engineers and of Electricians.

P S.—Since writing the above, I have received the enclosed letter from Mr. G. J. Symons, F.R.S., accompanying a parcel containing 60 important and valuable works on Lightning and Lightning Conductors, a list of which is also enclosed herewith.

A. J. F.

[COPY.]

"62, CAMDEN SQUARE, LONDON, N.W.,
"*Dec.* 19, 1881.

"A. J. FROST, Esq., *Librarian*,
"*Society of Telegraph Engineers*.

"DEAR SIR,

"Taking great interest in Scientific Bibliography, I naturally esteem very highly the services to science which your Society has rendered in issuing the Ronalds Catalogue, and in rendering the Library accessible.

"Finding that I have a good many books and papers on electrical subjects which are not in the Ronalds Library, and having (now that the Lightning Rod Conference has finished its work) no special need for them, I have decided upon offering them all to your Society, as at once a duty—that of helping, as far as in me lies, to perfect your splendid collection—and as a slight indication of my appreciation of the good work done by your Society.

"I believe the enclosed is a nearly perfect list of the contents of the parcel which will be sent to you to-morrow.

(Signed) "G. J. SYMONS."

LIST OF WORKS PRESENTED TO THE LIBRARY BY
G. J. SYMONS, Esq., F.R.S.

Instruction sur les Paratonneres adoptée par l'Académie Royale des Sciences, le 23 Juin, 1823. 8vo. 51 pp. 2 plates. Gay-Lussac, rapporteur. *Paris*, **1842**

Instruction sur les Paratonnerres du Louvre et des Tuileries. [*Ext. des Comptes Rendus*, tome lxvii. Séance 20 Juillet, 1868.] Pouillet, rapporteur. *Paris*, **1868**

Arago [F.] Ueber Gewitter. 12mo. 208 pp. *Weimar*, **1839**

Baudisius [A.] De lapide Fulminari. 4to. 24 pp. *Wittebergæ*, **1668**

Bertholon [de St. Lazare.] Die Electricität d. Lufterscheinungen. 2 vols. 8vo. *Leignitz*, **1792**

Borrel [A. P.] Instruction sur les Paratonnerres. 4to. 6 pp. and plates.

Brooks [D.] Atmospheric electricity. 8vo. 14 pp. *Philadelphia*, **1878**

Brough [R. S.] Protection of Buildings from Lightning. 4to. 24 pp. [*Lithographed*.] *Mussoorie*, **1878**

Buchanan [G.] An account of the chimney of the Edinburgh Gas Works. 8vo. 8 pp. [*Proc. Roy. Scot. Soc. Arts*.] *Edinburgh*, **1851**

Buchner [O.] De bliksemafleiders, door C. J. v. Doorn. 8vo. 131 pp. and plates. *Haarlem*, 1867

Chapman [Sir F. E.] Instructions as to the application of Lightning Conductors. 8vo. [*Army Circulars.*] *London*, 1875

Collin et Fils. Paratonnerres. [*Extract from Catalogue.*] 4to. *Paris*,

Dempp [K. W.] Vollständiger Unterricht in der Technik der Blitzableiter-setzung. 8vo. 110 pp. *München*, 1842

Edmonds [R.] On the two great thunderstorms, with agitations of the sea in 1846. 8vo. 5 pp.

Esebecius [J. S.] De fulmine et tonitru. 4to. 16 pp. 1659

Fonvielle [W. de.] Thunder and Lightning (translated by Phipson). 8vo. 216 pp. *London*, 1868

Fournet [J.] Sur la distribution des coups de foudre à Lyon. 8vo. 2 pp. *Lyon*, 1852

Gay-Lussac et Pouillet. Instruction sur les Paratonnerres adoptée par l'Acad. des Sciences. Part I., 1823, M. Gay-Lussac, rapporteur; Part II., 1854, and Part III., 1867, M. Pouillet, rapporteur. 12mo. 143 pp. *Paris*, 1874

Gray [J. W. & Son.] Lightning, its destructive action on buildings. 8vo. 15 pp. *London*, 1875

Hajingi [Bonno]. Disquisition de Fulmine Naturalis. 4to. 21 pp. 1660

Hamberg [H. E.] Om den s. k. luftelektriciteten. 8vo. 92 pp. *Upsala*, 1872

Harward [S.] A Discourse of the severall Kinds and Causes of Lightnings, written by occasion of a fearefull Lightning which on the 17th day of this instant Nouember, Anno Dom. 1606, did in a very short time burne vp the spire steeple of Bletchingley, in Surrey, and in the same melt into infinite fragments a Goodly Ring of Bells. 4to. 21 pp. *London*, 1607

Hemmer [J. J.] Verhaltungsregeln wenn man sich zur gewitterzeit in keinem bewafneten gebäude befindet. 8vo. 53 pp. *Mannheim*, 1789

Henry [J.] Directions for constructing Lightning Rods. 8vo. [Smithsonian Institution.] 4 pp. *Washington*, 1871

Hepburn [J. S.] Should Lightning Conductors terminate in a point or in a ball? (*Proc. Roy. Scot. Soc. Arts.*) 8vo. *Edinburgh*, 1855

Hugueny [F.] Le coup de foudre de l'ile du Rhin. 4to. 40 pp. [*Ext. Mem. Soc. des Sciences Nat. de Strasbourg.*] *Strasbourg*, 1869

Jarriant. Noveau Paratonnerre accepté par l'Académie des Sciences. 8vo. 8 pp. *Paris*, 1877

——— Etude sur les Paratonnerres. 8vo. 15 pp. *Paris*, 1878

Johnston [W. P.] Report on the Lightning Conductors at Dum Dum, Calcutta. 4to. 8 pp. *Calcutta*, 1878

Karsten [G.] Ueber Blitzableiter und Blitzschläge in Gebäude welche mit Blitzableitern versehen waren. 8vo. 20 pp. *Kiel*, 1877

——— Gemeinfatzliche Bemerkungen über die Elektricität des Gewitters und die Wirkung der Blitzableiter. 1st Edn. 8vo. 60 pp. *Kiel*, 1879

——— Ditto. 2nd Edn. 64 pp. *Kiel*, 1880

Koenig [J. G.] De Fulmine, Fulgure, ac Tonitru Hiemali. 4to. 16 pp. *Norica*, 1706

Krayenhoff [Baron.] Handleiding tot het stellen van Bliksemafleiders. 8vo. 61 pp. and plates. *Nijmegen*, 1826

Kuhn [K. G.] Bemerkungen über Blitzschläge. 8vo. 29 pp. [*Aus. d. Sitzuns. d. k. b. Akad. d. Wiss.*, 1867, B. II., H 42.] *München*, 1867

Langlois [E. H.] Notice sur l'incendie de la Cathédrale de Rouen occasionné par la foudre, le 15 Septembre, 1822. 8vo. 163 pp. *Rouen*, 1823
[This Cathedral is reported to have been struck by Lightning in 1110, 1117, 1284, 1351, 1625, 1627, 1642, 1768, and 1822.]

Laue [J. G.] De telo fulmine. 4to. 20 pp. *Lipsiæ*, 1706

Leigh [J.] Directions for Insuring personal safety during storms, and for the right application of Lightning Conductors. 12mo. 6th Edn. 60 pp. *London*, 1835

Majendie [V. D.] Report on the destruction by Lightning of a Gunpowder store. Fcp. fol. [Official Report—unpublished.] 5 pp. *London*, 1878

Mohn [H.] Lynildens farlighed i Norge. 4to. 22 pp. *Kristiania*, 1875

Monnet [P. A.] Nouveau procédé pour étudier l'électricité atmosphérique. 8vo. 12 pp. *Lyon*, 1865

Murray [J.] A Treatise on Atmospheric Electricity. 8vo. 149 pp. *London*, 1830

Nollet [J. A.] Vergleichung der Würkungen des Donners mit den Würkungen der Electricität. 8vo. (*Mém. de Paris*, 1764.) 126 pp. *Prag*, 1769

Peltier [J. C. A.] Sur le trombe de Monville (clivage des arbres par la foudre). 4to. 5 pp. *Rouen*, 1845

Perrin [P.] Etude sur les Eclairs. 8vo. 108 pp. *Paris*, 1878

Phillips [R.] On atmospheric electricity. 8vo. 57 pp. *London*, 1863

Plieninger [Dr.] Über die Blitzableiter, ihre Vereinfachung und die Verminderung ihrer Kosten. Nebst einem Anhang über das Verhalten der Menschen bei Gewittern. Eine gemeinfassliche Belehrung für die Verfertiger der Blitzableiter, sowie für die Hausbesitzer. Im Auftrage der k. Centralstelle des landwirthschaftlichen Vereins in Würtemberg. 8vo. 114 pp. 3 plates. *Stuttgart*, 1835

Poey [A.] Des caractères physiques des éclairs en boules. 8vo. 7 pp. *Paris*, 1855

—— Sur les éclairs sans tonnerre observés à la Havanne, pendant l'année, 1859. 8vo. 3 pp. *Paris*, 1860

Rédares [C.] Histoire abrégée du tonnerre. 4to. 16 pp. *Avignon*, 1853

Reussius [J. A.] Exercitatio Physica de Fulmine. 4to. 24 pp. 1676

Simmons [J.] An essay on the cause of lightning. 8vo. 81 pp. *Rochester*, 1775

Tessier. Observation sur l'effet du tonnerre à Rambouillet. 4to. 4 pp. *Paris*, 1785

Tomlinson [C.] The Thunder Storm. An Account of the Properties of Lightning, and of Atmospheric Electricity in various parts of the World. 8vo. 348 pp. *London*, 1859

Tomlinson [C.] On Lightning Figures. (*Ed. New Phil. Jour.*) 8vo. 11 pp. *London*, 1861

——— Further Remarks on Lightning Figures. (*Ed. New Phil. Jour.*) 8vo. 8 pp. *London*, 1862

Waelput [O.] Construction et placement de Paratonnerres. 8vo. 1st Edn., 11 pp.

——— Ditto 2nd Edn. 12 pp.

Weber [L.] Berichte über Blitzschläge in der Provinz Schleswig-Holstein. 8vo. 25 pages. 2 plates. 1880

——— Berichte über Blitzschläge in der Provinz Schleswig-Holstein. 8vo. 70 pages. 2 plates. *Kiel*, 1880

Wilson [R.] Boiler and Factory Chimneys, and on Lightning Conductors. 8vo. 64 pp. *London*, 1877

Zenger [Prof.] Symmetrische Blitzableiter. 4to. 2 pp.

A. J. Frost,
Librarian.

LIST OF ACCESSIONS TO THE LIBRARY DURING THE YEAR 1881.

*Works marked thus * have been presented by their Authors.*
„ „ „ † *have been purchased.*

* **Allard** [E.] Mémoire sur les phares électriques. La. 4to. *Paris*, 1881

* **America** [United States of]. Official Gazette of the United States Patent Office. 4to. *Washington*, 1881
By Exchange.

* **Anderson** [R.] On Lightning Conductors and Accidents by Lightning. 8vo. 8 pp. [*British Association, Dublin, Aug.* 20, 1878.] *London*, 1878

* ——— On the Necessity for a Regular Inspection of Lightning Conductors. 8vo. 10 pp. [*British Association, Swansea, Aug.* 27, 1880.] *London*, 1880

* ——— Information about Lightning Conductors, issued by the Academy of Sciences of France. 4to. 63 pp. [Translated by R. Anderson.] *London*, 1881

Angot. [*Vide* Cazin & Angot.]

† **Annales Télégraphiques.** 8vo. 480 pp. *Paris*, 1874

† **Armengaud** [ainé]. Manuel de l'éclairage électrique. 8vo. 232 pp. *Paris*, 1881

† **Armengaud** [Becquerel, and others]. L'électricité et ses applications 8vo. 174 pp. *Paris*, 1881

* **Ayrton** [W. E.] F.R.S. The Future of Mechanics' Institutions. 8vo. 4 pp. [*Delivered at the Manchester Mechanics' Institute, Oct.* 22*nd*, 1880.] 1880

* **Ayrton & Perry.** The Efficiency of Electric Lamps. [*Engineer, Nov.* 25, 1881.] Fol. 4 pp. *London*, 1881

* ——— Note on the Index of Refraction of Ebonite. 8vo. 4 pp. [*Phil. Mag., Sept.*] *London*, 1881

* ——— Note on Professor Exner's Papers on Contact Electricity. 8vo. 12 pp. [*Phil. Mag., Jan.*, 1881.] *London*, 1881

* **Ayrton & Perry.** A Portable Absolute Galvanometer for Strong Currents, and a Transmission Dynamometer. 8vo. 9 pp. [*Jour. Soc. Tel. Eng., May*, 1881.] *London*, 1881

* **Bank of England.** Catalogue of Library. 8vo. 285 pp. *London*, 1881
By Exchange.

† **Barbosa** [José Casas] Manual de Electricidad popular. 8vo. 240 pp. *Madrid*, 1881

* **Blavier** [E. E.] Des Grandeurs électriques et de leur mesure en unités absolues. 8vo. 586 pp. *Paris*, 1881

† **Boudet** [de Paris]. Des applications du téléphone et du microphone à la physiologie clinque. 8vo. 169 pp. *Paris*, 1880

† **Boulengé** [P. Le]. Description et emploi du chronographe le Boulengé. 8vo. 64 pp. 2nd Edition. 4 plates. *Bruxelles—Paris*, 1877

Brasseur. [*Vide* Sussex, S. W. de.]

* **Bureau International des Administrations Télégraphiques** [*Berne*]. La législation Télégraphique. 8vo. 535 pp. *Berne*, 1876

* ——— Documents de la Conférence Télégraphique Internationale de Rome, 1872. Fcap. 4to. 688 pp. *Berne*, 1872

* ——— Documents de la Conférence Télégraphique Internationale de St. Petersbourg. Fcap. 4to. 677 pp. *Berne*, 1876

* ——— Documents de la Conférence Télégraphique Internationale de Londres. Fcap. 4to. 667 pp. *Berne*, 1880

* ——— Nomenclature des Bureaux Télégraphiques. 5th Edit. 4to. 224 pp. *Berne*, 1880.

* ——— Statistique générale de la télégraphie dans les different pays de l'ancien continent. 4to. *Berne*, 1871—1881

Cantoni. [*Vide* Rossetti and Cantoni.]

* **Cappanera** [R.] Sopra un fenomeno di elettricità naturale. Sm. 8vo. 20 pp. [*Est dall Annuairo della Sezione Alpina Lucana, N.* 1, 1880.] *Potenza*, 1881

† **Cassell's Family Magazine.** A Family of Inventors. The Brothers Siemens. Part 80. 4to. 5 pp. *London*, 1881

† **Castelnau, Curières de.** Notice sur les signaux électriques désignés sous le nom de "Cloches Autrichiennes." 8vo. 24 pp. Plates. [*Est. des Ann. des Mines, Nov.-Dec.* 1880.] *Paris*, 1880

† **Cazin et Angot.** Traité theorique et pratique des piles électriques. 8vo. 311 pp. *Paris*, 1881

* **Chinese Government.** Proclamation relating to the first Telegraph line built by the Chinese Government. 1 sheet.
Presented by J. A. Betts, Esq.

† **Chrétien** [J.] Chemin de fer électrique des Boulevards à Paris. 4to. 58 pp. *Paris*, 1881

* **Clark** [Edwin]. Inaugural Lecture, given at the Town Hall, Great Marlow, Dec. 12, 1853. 8vo. 15 pp. 1853

* **Clark** [Latimer]. List of a Selection of Works relating to Electricity and Magnetism, exhibited at the Exposition Internationale d'Electricité, Paris, 1881. 8vo. 10 pp. *London*, 1881

* **Clark, Muirhead, & Co.** Instructions regarding Muirhead's Artificial Cable, and the Mode of Adjustment of the same in Duplex Working on Submarine Cables. 8vo. 9 pp. Plates. *London*, 1881

† **Clouet and others** [rapporteurs]. Rapport général sur l'éclairage électrique des Quais de Rouen. 4to. 54 pp. *Rouen*, 1881

† **Colladon** [D.] Mémoire sur les effets de la foudre, &c. 4to. 84 pp. *Genéve*, 1872

* **Congrès International d'Electricité.** Projet de Programme pour les séances. Sm. fo. 8 pp. *Paris*, 1881

Curieres de Castelnau. [*Vide* Castelnau.]

* **Dambrach** [O.] Le Droit Télégraphique. 8vo. 72 pp. *Berne*, 1872
Presented by the Bureau International Administration Télégraphiques.

* **D'Amico** [Commandeur]. Relazione Statistica sui telegrafi del Regno d'Italia, 1879. 4to. 68 and 198 pp, *Firenze*, 1880

De la Rue. [*Vide* La Rue.]

† **Déschanel** [A.] Elementary Treatise on Natural Philosophy. Part III., Electricity. Sixth Edition. 8vo. 314 pp. *London*, 1881

* **Du Moncel** [Th.] Notice sur la vie et les travaux de M. Gaugain. 8vo. 92 pp. Portrait. *Caen*, 1880

Edison. [*Vide Scribner's Monthly.*]

* **Engineering** [Reprinted from]. Heinrich's System of Electric Lighting. 4to. 10 pp. *London*, 1881

* **Engineers, Society of.** Transactions for 1873, 1874, and 1880. 8vo. *London*, 1874, 1875, 1880

* **Exposition Internationale d'Electricité.** Catalogue général officiel. 8vo. 229 pp. *Paris*, 1881

* ——— Catalogue Officiel. Section Belge. 8vo. 55 pp. *Bruxelles*, 1881

——— [*Vide* also Nyström.]

† **Evans and Smith.** On the Magnetic Character of the Armour-plated Ships of the Royal Navy, and on the Effect on the Compass of particular arrangements of Iron in a Ship. 4to. 62 pp. [*Phil. Tran.*] *London*, 1865

* **Faija, Hy.** Portland Cement for Users. 8vo. 97 pp. *London*, 1881

† **Faraday** [Michael], D.C.L., F.R.S. A course of Six Lectures on the Chemical History of a Candle, to which is added a Lecture on Platinum. Edited by Wm. Crookes. 8vo. 208 pp. *London*, 1861

† **Fournier** [George]. Receuil général de tous les brevets d'invention ayant trait à l'électricité pris en France depuis le 7 Jan. 1791, jusqu' à ce jour. 8vo. 221 pp. *Paris*, 1881

† **Fraser's Magazine.** The Electric Telegraph. By Dr. J. H. Japp. 8vo. 13 pp. [Oct. 1881.] *London*, 1881

Garnett [Wm.] [*Vide* Maxwell J. Clerk.]

Gaugain. [*Vide* Du Moncel.]

† **Gentlemen's Magazine.** A First Electrician. By B. W. Richardson. 8vo. 17 pp. [Oct. 1881.] *London*, 1881

* **Gilliland Electric Manufacturing Co.** Catalogue. 8vo. 15 pp. *Indianapolis*, 1881

* **Gore** [G.] LL.D., F.R.S. Thermo-electric Behaviour of Aqueous Solutions with Mercurial Electrodes. 8vo. 11 pp. [*Proc. Roy. Soc., No.* 199. 1879.] *London*, 1879

* ——— On Some Properties of Anhydrous Liquefied Ammonia. 8vo. 9 pp. [*Proc. Roy. Soc., No.* 141.] *London*, 1873

* ——— Chemico-Electric Relations of Metals in Solutions of Salts of Potassium. 8vo. 12 pp. [*Proc. Roy. Soc., No.* 200. 1879.] *London*, 1879

* ——— Influence of Voltaic Currents on the Diffusion of Liquids. 8vo. 30 pp. [*Proc. Roy. Soc., No.* 213. 1881.] *London*, 1881

* ——— Phenomena of the Capillary Electroscope. 8vo. 19 pp. [*Proc. Roy. Soc., No.* 212. 1881.] *London*, 1881

† **Harper's Monthly.** The Telegraph of to-day. 8vo. 15 pp. Oct. 1881. *London*, 1881

Heinrichs [*Vide Engineering.*]

† **Helmholtz** [H.] Popular Lectures on Scientific Subjects. 1st Series. 8vo. 348 pp. [Translated by Dr. E. Atkinson.] *London*, 1881

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A. J. FROST, *Librarian*.

The PRESIDENT: In asking you to propose that the reports be adopted and printed as part of our proceedings, I think I can fairly congratulate the Society on its present position and prospect of future success; and I may also refer to the very satisfactory state of the Library, and the results that have followed upon the zeal with which our Librarian has looked after its interests.

Mr. M. F. ROBERTS: I have great pleasure in proposing that the report which has just been read be adopted, and I am sure every member present cannot but consider it most satisfactory, and that it augurs well for the future prosperity of our Society.

I should, however, like, with the President's permission, to take this opportunity of referring to the method followed in the election of new members to fill vacancies in the Council. The members have all doubtless remarked that, in this year's balloting list, just so many names have been nominated by the Council as are required to fill the vacancies, thus practically making the

ballot which is now going on almost a matter of form. In referring to this question, I cannot but say that I think the new names which have been proposed could not possibly be improved upon, and in this view, doubtless, every gentleman present will agree—(hear, hear)—but I think it would certainly tend to the well-being of the Society if, in future, the Council put forward, say, double the number of names required, and members would then have an opportunity of exercising some selection, and of making the ballot more than a mere matter of form. At present it would be almost impossible, in the few days that elapse between the time when the list of the proposed new members is circulated and the day on which the ballot takes place, for a sufficient number of members to arrange to vote for another gentleman in place of one of the names submitted, however desirable they might think such a change would be. I must certainly say, however, in the present case, that I do not think any change in the names proposed for election could possibly be an improvement; but this very fact has led me to make these remarks, and has enabled me to do so without the possibility of a suspicion that, in saying what I have, there was any personal objection to any of the gentlemen nominated. I hope the Council will see no objection to the course I propose being followed in future. I have pleasure in again proposing the adoption of the report.

The PRESIDENT: The question referred to by Mr. Roberts, as to whether a larger number of names than of vacancies should be given in the balloting list, has been very carefully considered by the Council, and has not escaped their attention in any degree. The point was considered of much importance, and after many discussions, in which various opinions were expressed, it was decided by a considerable majority that the smaller evil was to nominate a number of names corresponding to the number of vacancies. It is known that, in some cases in which the nominations exceeded the vacancies, annoyance has been caused to gentlemen whose names have been suggested by the Council, but not adopted by the Society. On the one hand, there is the danger of causing difficulties of this kind; while, on the other, there is the evil that

members of the Society may feel that they are deprived of free choice by the plan of nomination that has been adopted. A similar plan is followed by a large number of societies, including the Royal, Physical, Chemical, Linnæan, Geological, and a great many more, but of course it is in the power of this Society to lay down any rule it may please. At present no fixed rule exists, though there has been a prevailing usage. It is felt by the advocates of the present plan that the Council probably have a better opportunity of judging of the wants of the Society, and of choosing for nomination gentlemen who would represent the various classes of our members, than perhaps any individual member, or even any group of members. The Council have acted to the best of their judgment: they may have made a mistake, but they have certainly not desired in any degree to fetter the free expression of the wishes of the Society.

Mr. H. R. KEMPE seconded the proposition. He remarked that it would be a great advantage to the electric world generally if in connection with the Library a catalogue of patents referring to electricity were provided. The Patent Office record referred to patents generally, and great difficulty was felt in searching out particular electric patents, which would not be the case if a catalogue, as suggested, were compiled and kept up to date at the Library.

The proposition was carried.

Mr. J. LAISTER proposed the following resolution :—" That this meeting resolves unanimously that a cordial vote of thanks of this Society be presented to the Members of Council and Members of the Institution of Civil Engineers, for their kindness in continuing to permit the general meetings of the Society to be held in the theatre of the institution," and said that such a resolution must commend itself to all the members of this Society. Every year it became less and less a formal matter to pass this resolution, for every year the obligation to the Institution for its extended courtesy became more and more substantial, and demanded that our thanks be given with increased heartiness.

Mr. F. HIGGINS seconded the proposition, which was carried most heartily.

M. R. VON TREUENFELD proposed a vote of thanks to the Honorary Secretaries for the performance of their duties during the past year. Having recently returned from South America, M. Treuenfeld said he was able to say, of his own knowledge, that, owing to the distance (in some cases hundreds of miles) that members were separated, it was no easy matter for the local Honorary Secretaries to thoroughly look after the interests of the Society.

Mr. C. E. SPAGNOLETTI seconded the vote of thanks, and referred to the exceptionally heavy duties which were liable to, and do at times, fall upon local Honorary Secretaries and Treasurers, as in the case of the recent Paris Exhibition, to the success of which, as far as the British section was concerned, the untiring zeal and energy of Mr. J. Aylmer so largely contributed, as well as to the satisfaction and comfort of the exhibitors, especially to those who were unable to stay there all the time.

The vote was carried.

Lieut.-Col. J. U. BATEMAN-CHAMPAIN, R.E., moved: "That the best thanks of the Society are due to Mr. Bristows, Honorary Solicitor, for his kind services, involving, as they have done during the past year, the occupation of much valuable time." With reference to the duties performed by the Honorary Solicitor, Lieut.-Col. Champain said that, without Mr. Bristows' courteous and skilful aid, the difficult problems met with in the process of incorporation of the Society, now being gone through, would have seriously embarrassed the Council.

Dr. J. HOPKINSON seconded the proposition.

Mr. C. F. VARLEY: I have known Mr. Bristows ever since 1846. Messrs. Wilson and Bristows were solicitors to Messrs. Cooke and Wheatstone before any Telegraph Company existed: they in part helped all the world to telegraphs, and I have very great pleasure in seconding the resolution.

The PRESIDENT: I am informed by the scrutineers that the following gentlemen have been elected as Officers of the Society for the following year, viz. :—

President.

Lieut.-Col. C. E. Webber, R.E.

Vice-Presidents.

Willoughby Smith. | Professor W. G. Adams, F.R.S.
C. E. Spagnoletti, M. Inst. C.E. | Professor D. E. Hughes, F.R.S.

Honorary Treasurer.

Mr. Edward Graves.

Honorary Secretary.

Lieut.-Col. Frank Bolton.

Members of Council.

W. S. Andrews.
W. T. Ansell.
Sir C. T. Bright, M. Inst. C.E.
G. B. Bright.
H. G. Erichsen.
H. C. F. Forde, M. Inst. C.E.
Alexander Siemens.
Augustus Stroh.
C. F. Varley, F.R.S.
Earl Crawford & Balcarres, F.R.S.
Matthew Gray.
J. F. Moulton, F,R.S.

Associate Members of Council.

Capt. Bicknell. | T. R. Crampton.
C. H. B. Patey.

A vote of thanks was passed to the scrutineers.

The following paper was then read :—

ON A CONVENIENT FORM OF CONSTANT BATTERY OF VERY SMALL INTERNAL RESISTANCE.

By Cromwell F. Varley, F.R.S.

In 1854, I obtained Letters Patent, No. 2,555, for Improvements in Voltaic Batteries in which neutral salts were used, the two solutions being kept apart by the action of gravitation: hence the battery has been called the gravity battery. In the before-mentioned patent, I pointed out that, when powdered burnt clay or other porous substance was used, the solutions were more easily kept apart without the resistance being greatly increased.

The Electric International Telegraph Company used large numbers of these cells. They were constructed as follows:—

A plate of lead was attached to a copper wire, and this plate was rather more than twice the length of the bottom of the square battery cell. The lead was first bent at right angles, so that it should lie at the bottom of the cell. On this was placed 2 lbs. of sulphate of copper in crystals. The upper portion of the lead was then bent down over the crystals of sulphate of copper. On the top of this lead was placed some pulverised burnt clay, sometimes unburnt clay. The zinc being placed over the lead plate at a distance of about 2 inches, the cells were filled with a quarter saturated solution of sulphate of zinc. This form of gravity battery received the cognomen of "Varley's mud battery."

The zinc plates were cast in the form of a very flat pyramid, with the apex downwards. This is necessary to enable the bubbles of gas which rise to escape from the surface of zinc, otherwise they cause a very considerable increase in the internal resistance of the battery. Owing to the comparatively small resistance of this form of neutral salt Daniell's battery, they received another cognomen, namely, "quantity battery."

The battery in this form was patented by Menotti, about the year 1861. He used sand to separate the solutions. The sand had been replaced by sawdust by Sir William Thomson in 1858, who patented it. The gravity battery with sawdust usually passes under the name of Menotti, notwithstanding that my above-named patent has been very well known to those who have used the latter name.

In the same patent will be found, both in the specification and in the drawings, a battery made with sulphate of mercury, since so largely known as the Marié Davy battery.

The battery itself is a modification of the constant battery so justly associated with the name of the late Professor Daniell, and is generally known as the Daniell's battery. He, however, made use of fluids strongly impregnated with sulphuric acid, which reduced the internal resistance prodigiously. The porous cells or diaphragms themselves add largely to the internal resistance of the battery.

Strong sulphuric acid is not a good conductor of electricity, but when diluted its conducting power rapidly increases up to a certain point, when it slowly decreases again. Strong sulphuric acid will not dissolve sulphate of copper. The commercial crystals of the latter consist of one atom anhydrous cupric sulphate, specific gravity 159·4, combined with five atoms of water: in round numbers, sixteen of anhydrous cupric sulphate to nine of water by weight.

When blue crystals of sulphate of copper are placed in strong sulphuric acid, the acid has so great an attraction for the water of crystallisation that it actually pulverises the crystals of cupric sulphate, leaving the anhydrous salt in a greenish white amorphous powder.

Sulphuric acid, in round numbers, is nearly twice as heavy as water, its specific gravity being 1·842, and when the strong sulphuric acid, which consists of one atom of sulphuric anhydride combined with one atom of water, is mixed with twice its volume of water, instead of three volumes being obtained, the whole is condensed into a much smaller compass. This condensation renders this solution much heavier than it would otherwise be.

The acid has now become a powerful conductor of electricity, but if two more volumes of water be added it attains to its highest conducting power. When one part of acid is combined with eight volumes of water, the conducting power of the weaker acid is equal to that of the acid consisting of one volume of acid added to two volumes of water; but with sixteen volumes of water its conducting power is still great.

In the remainder of the paper I will use the expressions "strong acid" and "weak acid," to express these two solutions. If some of the strong acid be placed into a glass or other jar, and the weaker acid be poured on top of it carefully by means of a funnel having a flat disc underneath it, the two solutions will remain separate, by gravitation alone, for a considerable time.

Prior to pouring in the strong solution, a plate or coil of copper, to which a stout copper wire has been riveted, is inserted in the jar, to form the negative pole or plate. When the weak solution has been poured in, a rod or cone of zinc, well amalgamated, is

suspended in any convenient manner in the weaker solution, at a distance of one or more inches above the copper. The battery is now ready for use. It is, in fact, an acid Daniell battery without a porous cell.

In order that the eye may see how the acids diffuse, I colour the strong solution with indigo, or litmus, or red cabbage. When the battery is *in situ*, the strong acid is poured in. This acid I colour with sulphate of indigo; a brass or glass tube is now inserted, and through it are thrown crystals of sulphate of copper, to dehydrogenise the copper surfaces.

I will now describe an expedient to which I resorted in a country inn, being suddenly called upon to procure an electric current for an investigation upon the spot.

The chemist had blue stone and oil of vitrol: the plumber had copper bell wire and thin sheet zinc. The chemist, luckily, was provided with mercury. The inn supplied me with tumblers and water. Some pieces of copper wire were twisted up into spirals at one end, to form the negative plate at the bottom of the tumbler; the wire coming from that was bent sharply over the edge of the glass and then up again, to hold it *in situ*.

On the end of each wire was soldered one of the slips of zinc, and this was afterwards amalgamated. The two solutions of sulphuric acid were prepared and allowed to cool, the tumblers and metals were arranged in order on a piece of common deal board, and the terminal copper and zinc were fixed to the board by means of nails. In this way, ten elements were soon extemporised, each having a resistance of 0·5 ohm.

When in process of time the sulphate of copper is seen to be rising, all that is necessary is to fill up the tumbler with diluted acid, insert the nozzle of a syringe down to within an inch of the copper plate or coil. On withdrawing the fluid, the intermediate portion, consisting of a weaker acid than the strong, but a stronger acid than the weak solution, is withdrawn, leaving the line of demarcation between the coloured sulphate of copper solution in the strong acid and the transparent weaker solution sharp and well defined.

In process of time it becomes advisable to lower a funnel to the

bottom of the vessel, and admit a small quantity of the strong acid solution previously cooled.

In this way the battery may be kept going for months, without having to remove the metals. Sulphate of copper must from time to time be added in crystals, and this may be done by dropping a large dry crystal of sulphate into the vessel from time to time, through a piece of tubing.

This battery is so small in internal resistance, is so simple of construction, and so costless in manufacture, that it will be found of great service to electricians in all ranks of life, especially when they suddenly want a battery in out-of-the-way localities. I have therefore thought that this description will be of value to the members of the Society of Telegraph Engineers and of Electricians.

In Patent 2,555 of 1854, I pointed out a method of preventing the sulphate of copper from reaching the zinc plate.

In 1864, 1865, and 1866, when testing the Atlantic Cables, I used at the Wharf Road, City Road, Gutta Percha Works, 500 cells of battery, and at the Cable Works at Greenwich, another set of 500 cells, constructed in the following manner:—

A ring of copper was placed at the bottom of an earthen jar. About 1 lb. of crystals of sulphate of copper were placed inside the copper ring. A layer of 1 inch of prepared sawdust was next laid on the copper crystals. Upon this was placed half an inch of oxide of zinc, and again upon this was placed about an inch of the prepared sawdust. Upon this upper layer of sawdust a heavy zinc mass with a conical bottom rested. The sawdust was prepared by first boiling in a caustic soda to dissolve the resin. It was then well washed in water, and lastly in a diluted solution of sulphate of zinc, consisting of one part saturated solution to three of water. When the zinc plate had been placed *in situ* the whole was moistened with sulphate of zinc. The object of the layer of oxide of zinc is to arrest the sulphate of copper which slowly diffuses upwards towards the zinc. The sulphuric acid of the sulphate of copper is seized upon by the oxide of zinc forming sulphate of zinc, while the oxide of copper so formed replaces the oxide of zinc.

The deposition of metallic copper upon the zinc not only lowers

the potential of the battery, but makes its action very irregular. The layer of oxide of zinc prevents this, and, if the zinc plate be well amalgamated, the battery will seldom be found to vary more than 1 in 300. This battery is very constant. Its only inconvenience is, that after the lapse of two or three months the internal resistance becomes nearly doubled by the deposition of a compact layer of oxide of copper.

I have used 1,000 cells of this battery almost continuously since the year 1858. I place a glass tube in the cell, to admit of the introduction of crystals of sulphate of copper from time to time. I seldom have to dismantle the battery more than once in the course of a year.

The PRESIDENT: As a convenient form of battery with low resistance and high electro-motive force is what we are all wishing for, I am sure we are glad to hear remarks on the subject from one who has had so great experience as Mr. Cromwell Varley. I should like to ask Mr. Varley if the zinc in the battery he has just described is cast, and whether such material has proved by his experience better than rolled zinc for batteries.

Mr. C. F. VARLEY: It is cast zinc, well amalgamated.

Dr. WALTER COFFIN remarked that ten years ago, as described in the "Chemical News" for November 17th, 1871, page 231, he experimented with sulphate of mercury gravity cells, as described by Mr. Varley, and found that, owing to the slow solubility and diffusion of the salt, considerable variation occurred on short circuit, otherwise it was a very useful and convenient battery, from the fact that the basic metal supplied as the negative element produced no local action if reduced upon the zinc. In the same communication to the "Chemical News," he (Dr. Coffin) described a battery in which mercury chloride (calomel) was used for the same reason; a solution of hydrochloric acid was employed, and to increase the conductivity of the chloride it was mixed with crushed carbon, and very constant results were obtained, nearly equal to that of chloride of silver. In his opinion, the principle of employing as the negative element a material which, when reduced, should improve the zinc instead of increasing

local action, was interesting and important, and with high external resistance rapid diffusion was not necessary to give constant results.

Mr. C. F. VARLEY: I can confirm Dr. Coffin's remarks. The use of bichloride of mercury is very good in a battery, and is preferable to chloride of silver.

Mr. H. R. KEMPE enquired whether the wedge shape of the zinc in Mr. Varley's battery was for the purpose of enabling the hydrogen gas to disengage freely. The common practice of making batteries with flat plates was, he believed, thoroughly efficient; and he asked whether the wedge form was the result of experiments.

Mr. C. F. VARLEY: I do not think any person has yet satisfactorily explained why hydrogen gas should be given off in that form of battery, but I have proved by analysis that it is so. If you have a plate as shown in the diagram, a gas will form underneath it, and the presence of that gas causes resistance. I put the zinc in the form shown in the diagram in my early patent (and it has always been used in that form), in order that the copper which *will* diffuse and *will* deposit on the zinc shall drop off again; but, without anticipating it, I found that the shape was good for letting off the hydrogen as well, and since then I have always used that form. I always have 2,000 cells charged at my house, and they work very well indeed; they cause no trouble, and I see no reason why I should alter them. They have been in work since 1854: they let off the gas, drop off the copper, give off plenty of power, and work very well, and I do not know what I could do to make any better arrangement.

Mr. F. HIGGINS: If I understand the description of the battery correctly, it is composed of a lot of sulphate of copper in the bottom of the cell, with an amalgamated zinc plate at the top, and a sulphuric acid solution between them. My experience with such an arrangement is, that after a time a portion of sulphate of copper reaches the surface of the zinc plate, producing a copper amalgam upon it, which would create local action, and tend very soon to destroy the efficiency of the battery. Has Mr. Varley made any arrangement to prevent the rising of the sulphate of copper, such as I have described?

Mr. C. F. Varley: If you keep your zinc well amalgamated, it takes the potential, not of the copper, but of the most oxidisable metal; and it will always be found that the potential of the zinc, so long as it is well amalgamated, is that of zinc and not sulphate of zinc. The same rule holds good with sodium, the only solution I know of which allows a difference is sugar of glycerine, when twice the potential is obtained, but it does not make a practicable battery.

Mr. F. Higgins: Certainly we have to learn as we go on, but I have tried a great many batteries in which there have been zinc plates, well amalgamated, with copper plates in sulphate of copper solution, and after a time a little sulphate of copper has got over to the zinc, and a few spiculæ of copper have been precipitated upon the amalgamated surface of the zinc, and nothing you can do will cause these particles of copper to lie down sufficiently flat to prevent a current of hydrogen gas being given off from their points. I do not know how that difficulty is to be got over. I have made a great number of batteries with amalgamated zinc plates opposite copper plates, and the copper invariably deposits a point or points from which the hydrogen is given off in continuous streams. A battery getting over that difficulty must be very efficient indeed; a gravity battery like the one shown on the diagram, with plates about six inches inches in diameter and about an inch apart, would give an immense volume of current, and be very useful in most telegraphic operations. But I have invariably found that the local action was enormous. If brushed over occasionally, to cause the amalgamation of the precipitated copper, this destructive action is somewhat checked, but is still very great.

Professor Ayrton drew Mr. Higgins' attention to Mr. Varley's description of the use of the syphon, by which he prevented the copper in solution from reaching the zinc, by drawing it away.

Mr. Higgins: Of course, if that be so, the difficulty I mentioned is disposed of; but I repeat that if the copper once reaches the zinc, is deposited on it, and is precipitated in a metallic form, it must result in local action.

Mr. C. F. Varley: When the copper rises up to the zinc, the zinc is well amalgamated; the zinc then, of course, becomes covered with a coating of copper and mercury. That action may go on for months without producing an appreciable effect, but when the

copper coating becomes thick the result is "polarisation." This action is prevented by using the syringe as I have stated, and keeping the solutions well apart.

Professor AYRTON remarked that some years ago, when in Western America, he was shown a form of battery in which two spirals of copper twisted in reverse directions were used, one above the other, and he was told that the magic of its action depended on the spirals being wound in opposite directions, a conclusion about which he was naturally dubious. Such an arrangement suggested to him the possible advantage of using two plates. He took a copper box, in the top of which a few holes were made to allow the solution to pass through, and the difficulty he found was that the top of the box would become eaten away, possibly through the insufficient supply of sulphate near it—*i.e.*, as the sulphate of copper diminished in quantity, so the density of the sulphate at the top of the box decreased, and local action commenced between the top and bottom of the box. Mr. Varley had evidently found the same difficulty, but, by the arrangement of the cell described, did he propose to overcome it?

Mr. C. F. VARLEY: If you take a plate of copper and place sulphate of copper at the bottom in a jar, and sulphuric acid at the top, at the line of demarcation, there will be a slow deposit of copper on the lower part of the plate in the sulphate of copper. This ought not to occur, according to chemical theory, but it is a fact.

Mr. F. HIGGINS: The spiral battery referred to by Professor Ayrton is the "Lockwood" battery. There was no electrical virtue in the arrangement of the spirals, but the arrangement was convenient for withdrawing either of them when it became very much incrusted with a deposit of copper. Such cells are extremely efficient, and 20 of them, with 6 units internal resistance each, upon a circuit of 300 units resistance, will work for seven or eight months without being touched in any way whatever, and by that time the sulphate of zinc solution becomes so dense that, shortly after, it crystallises into an almost solid block. The reason why a copper plate is dissolved in sulphuric acid solution or sulphate of zinc solution, when in connection with a copper plate in sulphate of copper solution, is, because the copper plate in the sulphate of zinc solution

is electro-positive to the other, and acts something like a zinc plate would. It is only necessary for the precipitation of copper in solution to have a potential of about one-tenth of a volt, and there is very much more than that, there is about one-fifth of a volt, in an arrangement of the kind mentioned.

Dr. COFFIN referred to the great number of patents that had been taken out for the purpose of preventing the diffusion of the copper upwards, and said that in America, where closed circuits and gravity batteries are employed, it was found that, in spite of every such device, copper always gets to the zinc. Such action does not lower the potential of the zinc to any extent, and the precaution of hanging a second piece of zinc in the solution involved an economic fallacy; the only remedy in his opinion being the separation of the plates as far as possible, or the constant attention described by Mr. Varley.

A vote of thanks was passed to Mr. Varley for his paper.

A ballot for new members then took place, and the following were elected:—

As Foreign Members.

Monsieur de Bedemann.
Frank L. Freeman.
Charles F. Heinrichs.
Don Carlos de Orduña.

As Members.

William H. Davies.
Frederick N. Gisborne.
E. H. Pringle, F.R.C.S.

As Associates.

Thomas Anderson.
Professor W. F. Barrett.
Eustace W. Chetwynd.
C. H. Grace.
Charles G. Gümpel, A. Inst. C.E.
W. H. Massey.
Henry M. S. Mathews.
William Moon.
Fritz Cunliffe Owen.
William Picken.
F. W. Pope-Cox.
Frederick T. Rickards.
A. G. Dew Smith.
Major Stopford.
Frederick William Vane.

As Student.

Harold W. Ansell.

After which the meeting was adjourned until January 19th, 1882.

ORIGINAL COMMUNICATION.

PREPARATION OF THE PURE MERCUROUS SULPHATE FOR LATIMER CLARK'S CONSTANT CELL.

By T. Iwata, M.E.,

Instructor in Natural Philosophy and Telegraphy in the Imperial College of Engineering, Tokei, Japan.

As in countries like Japan, where pure chemicals are not easily purchasable, it is necessary, for the manufacture of Latimer Clark's constant cells, to prepare for oneself pure mercurous sulphate, the following two methods, originally suggested to me by the Professor, and which have now been employed for some years in the Physical Laboratory here, may be interesting to some of the members.

In making mercurous sulphate there are two distinct methods, which may be called the hot and the cold, and which are as follows:—

Hot Method.—Pour plenty of pure metallic mercury into an evaporating dish, and add to it distilled sulphuric acid, the quantity of which should not be more than the half of that of the mercury. Heat these up to about 210° C., and keep the temperature constant; after a while the mercury will be covered with a white shell, which is mercurous sulphate, or more generally a mixture of mercurous and mercuric salts. Care must here be taken not to convert all the acid into sulphate, otherwise most of the product will be mercuric sulphate. When the amount of the acid is very much diminished, say, to a tenth of the original, the source of heat should be taken away and the product allowed to become quite cold, when the sulphate should cautiously be removed to another evaporating dish, or, better, put into a bottle, care being

taken not to get out any trace of metallic mercury or the part of the sulphate touched to mercury, otherwise it is impossible to obtain clean white sulphate. The sulphate should be washed with clean pure water, and if, after decanting off as much water as possible, the colour of the sulphate changes into yellow, which is the sign of mercuric salt existing, the sulphate should be washed with distilled water until the colour of it becomes quite free from yellow. If the product contains so much mercuric salt that we are unable to get rid of it by washing it simply with distilled water, we must pass through the step of washing the product with dilute sulphuric acid, to purify it. For this purpose ordinary sulphuric acid may be used, provided the acid contains no trace of chloride.

Cold Method.—First prepare dilute nitric and sulphuric acids, which can be done simply by diluting ordinary acids, if the acids contain no trace of chloride.

Put the dilute nitric acid into a bottle containing plenty of pure mercury, and leave it at least for 24 hours. The yellowish white crystal will be found on the metallic mercury. Decant off all of the watery liquid, leaving the white crystal on the surface of the mercury. Washing the mercury, together with the crystal, with the dilute nitric acid, it will be seen that all, or nearly all, of the crystal is dissolved by the acid forming clear liquor, which should be decanted off. Adding the dilute sulphuric acid to the clear liquor, a white fine powdery precipitate will be produced, which is mercurous sulphate. Decanting off the clear liquor, and washing the precipitate with clean pure water, we get clean pure mercurous sulphate.

The time which is required for dissolving mercury in the dilute nitric acid varies according to the strength of the acid, so that it is desirable to use moderately strong dilute acid, unless it is so strong as to produce sensible heat when acting on the mercury. Usually we may safely use dilute nitric acid, consisting of one of acid to three of water by volume. As to the strength of dilute sulphuric acid, somewhat weak is preferable to stronger.

Of these two methods it is very advisable to employ the cold in preference to the hot one, since with the former the operation is

very much simpler than that involved in the latter. In reality it is nearly impossible to obtain pure mercurous sulphate by the hot method.

Tests for Purity of Mercurous Sulphate.—The sulphate should at first be converted into chloride by adding hydrochloric acid and allowing the mixture to stand without heating. Filter the liquor containing the grayish white powder, when the clear liquor should be treated (1) with protochloride of tin, and (2) with ammonium sulphide, previously neutralising the excess of acid with potassium hydrate. In the first case, if a white precipitate is formed, mercuric sulphate is present; and, in the second case, if a black precipitate is found, the salt will also contain mercuric sulphate as an impurity. These tests, however, are not necessary when the sulphate is produced by the cold method.

ABSTRACTS.

H. HELMHOLTZ—AN ELECTRO-DYNAMIC BALANCE.

(Annalen der Physik und Chemie, B. XIV., H. 1, No. 9, 1881, pp. 52-54.)

The author undertook the construction of an electro-dynamic balance, in order to eliminate the disturbances, produced by changes of direction and intensity of the earth's magnetism, in the measurements of galvanic currents by means of their electro-magnetic effects.

Two coils of copper wire, the height of which is equal to their internal diameter, are suspended from the arms of the beam of a small chemical balance, in place of the usual scale-pans. Their axes are placed vertically, and any rotation about these axes is prevented. Two larger coils, of the same height as the suspended ones, but of greater diameter, are fixed to a horizontal metal bar, which is attached at its centre to the pillar of the balance. The connections of the wires are so arranged that one of the suspended coils is attracted by one of the fixed coils, while the other suspended coil is repelled by the second fixed coil. Both the fixed coils are placed somewhat above the suspended ones, so that the attracted coil is raised, and the repelled coil descends when the current passes.

Two chief difficulties have to be overcome in the construction of such a balance. The connections for the movable coils must not interfere in any way with their freedom of movement; and yet weak contact springs cannot be employed, as they would cause changes in the resistance of the circuit. This difficulty was overcome in a very satisfactory manner by using Dutch gold for the connections. Strips of this about 30 cm. long and 6 to 7 mm. wide are very flexible, show no internal friction, have a very moderate resistance, and are not easily heated, owing to the large surface exposed to the air. Each of the movable coils was connected to the wires carrying the current by two such strips, hanging down from pieces of brass fixed on the upper part of the glass case of the balance.

The second difficulty is to place the coils in such relative positions that neither the stability nor the sensitiveness of the balance is interfered with. For this reason the intensity of the electro-dynamic force must not vary too much during the usually small oscillations of the balance. This force is nil when the middle of the movable coil is in the same plane with that of the fixed coil, and also when the coils are at an infinite distance from each other. Between these two positions there is one of maximum force, which nearly corresponds to that, in which the upper surface of one coil is in the same plane with the lower surface of the other. Between the central position and the position of maximum

effect, the differential coefficient of the force, for increasing distance of the middle points of the two coils, is positive; and it becomes negative when the position of maximum effect is passed. At an infinite distance it is again zero. Between the position of maximum effect and the infinite distance there must, therefore, be a position for which the negative differential coefficient of the force reaches a maximum, and the second differential coefficient is zero. This position is the one which must be given to the coils. As the distance of the one pair of coils is always diminished by just so much as that of the other pair is increased, the change in the force depends only on the second differential coefficient. If this is positive, the current produces a state of unstable equilibrium; if it is negative, the equilibrium is increased, *i.e.*, the balance is less sensitive when the current passes than when there is no current. If the coils are brought into the right position, neither the equilibrium nor the stability of the balance is altered, and in this way the correct position can be ascertained.

If the apparatus is well set up, the error which can occur is not greater than a milligramme. As the force exerted by the current is proportional to the square of the intensity, the intensity of a current, which is balanced by a gramme, can be determined within $\frac{1}{2000}$. The only force opposed to the electro-dynamic force, and which serves to measure it, is that of gravity, and no fluctuations are introduced into the measurement, such as might arise from the earth's magnetism, or from the elasticity of a unifilar or bifilar suspension.

The determinations of the electro-dynamic equivalent of a current corresponding with one gramme, made by several independent observers, showed a marked coincidence.

TEMPERATURE OF THE ELECTRIC ARC.

(*Elektrotechnische Zeitschrift, September*, 1881, *p.* 343.)

In 1860, Becquerel found that the temperature of an electric arc produced by 80 Bunsen cells was from 2,070 to 2,100 degrees cent., and he laid it down that the illuminating power of the arc increased in direct proportion to the amount of heat radiated, a law which had been already established by Dulong and Petit. Other observers having found that this law does not hold good for high temperatures, Rosetti undertook the investigation of the question. His experiments were carried out in a different manner to any tried before. He caused the heat rays, which emanated from a given surface of the carbons, to fall upon the surface of a thermopile in circuit with an astatic galvanometer. The results of a large number of experiments, made with 160 Bunsen cells and a Dubosq lamp, showed that the temperature of the positive carbon varied between 2,400 and 3,900 degrees cent. The smaller the radiating surface, the greater the heat, provided that the extreme point of the carbon was included in the surface observed. The temperature of the negative carbon varied between 2,138 and 2,530 degrees cent., but the extreme point had a temperature of not less than 2,500 to 3,900 degrees cent. In a Reynier lamp worked by 8 or 10 Bunsen cells, the positive carbon reached a temperature of 2,406 to 2,734 degrees cent.

AN ELECTRO-MAGNET OF UNUSUAL SIZE.

(*Elektrotechnische Zeitschrift, May*, 1881, *p*. 185.)

Professor von Frilitzsch and Dr. W. Holtz have constructed an electro-magnet for the Physical Institute of the Greifswalder University, which greatly exceeds in size any hitherto known. The core weighs 628 kgs., and consists of 28 bars of sheet-iron 7 mm. thick, which form a cylindrical horse shoe 195 mm. in diameter and 125 cm. high. To avoid the extra current, the plates are lacquered, and are held together by wrapping round them a hempen cord soaked in sealing-wax varnish, to ensure good insulation between the coils and the core. This core is built into an oak box with bricks and cement to such a depth that the legs of the magnet project 950 mm., the box being 870 mm. long, 255 mm. wide, and 295 mm. deep. The coils consist partly of strips of sheet-copper and partly of copper wire; 100 kgs. of copper strips are wound round the cylinder in rings, each of which has 15 turns, the successive layers being insulated from each other by strips of sheet gutta percha; their ends are soldered together so as to form one continuous circuit. Over the copper strips are wound 175 kgs. of copper wire, 2 mm. in diameter, partly insulated with wool soaked in shellac, and partly with cotton soaked in wax, the winding being so arranged that two wires are wound on together. In this way 5 double layers of 2 wires each are formed, the layers being separated by stout paper, and each having about the same resistance as the whole of the coils of copper strip. On the lid of the box is a commutator and an arrangement by which the several layers of wire can be connected up as required. The poles carry pole-pieces of 33 mm. strong iron plate, which can be moved as desired. The following table gives a comparison of electro-magnets of large size which have been constructed:—

Name.	Distance of poles. Mm.	Length of core. Mm.	Diameter of core. Mm.	Weight of core. Kg.	Section of wire. Mm.	No. of layers.	Weight of wire. Kg.
Faraday's Woolwich Bar	—	711	68·5	17·8	14·52	4	19·5
Faraday's Horseshoe ...	102	1,168	95·25	64·3	14·52	3	20·3
Plücker's Horseshoe ...	284	1,320	102	84	14·93	3	35
Greifswalder Horseshoe	596	2,706	195	628	6·28	25	275

F. VON HEFNER-ALTENECK—A NEW DYNAMO MACHINE FOR CONTINUOUS CURRENTS.*

(*Elektrotechnische Zeitschrift, May*, 1881, *p*. 163.)

In a paper read before the Elektrotechnischer Verein, the author gave an account of a new machine of his invention. He commenced with a short account of the alternate-current machine of Messrs. Siemens, from which the new machine is derived. On the bed-plate of the alternate-current machine are fixed two iron supports of a circular form. On the inner side of each of these supports are fixed an even number of electro-magnets. The winding of these electro-magnets is so arranged that each one has the opposite polarity of the one facing it, and of those right and left of it on the same side of the machine.

* The abstract of this paper is given somewhat fully, since the accounts of this machine that have already appeared in English periodicals have been more or less inaccurate.

Through the magnetic fields thus produced induction coils are made to rotate. Two of these coils are shown in the figure, and, for the sake of distinct-

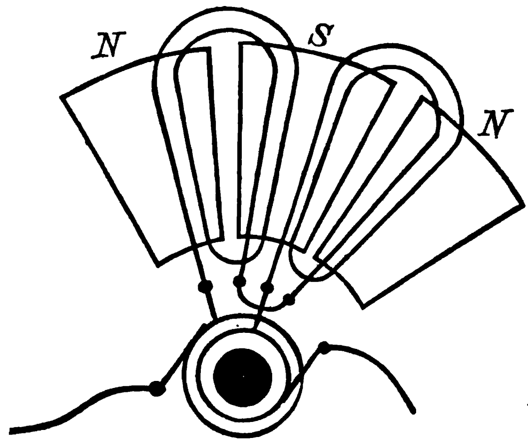

FIG. 1.

ness, with only two layers of wire on each coil, at the moment when the one-half of the coils are between one pair of electro-magnets, and the other half between the next pair. Thus in the one half of the coil an upward current is produced, and in the other half a downward current, and these two currents

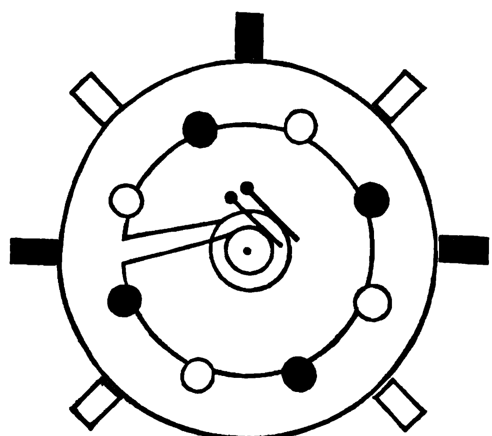

FIG. 2.

reinforce each other. When by the continuance of the rotation the coil comes just between two electro-magnets, induced currents are set up in both halves of the coil, and a little further on the two opposite currents again reunite, and so on.

As the number of coils is equal to that of the magnetic fields, the current is simultaneously reversed in all the coils. In consequence of the opposite polarity of two adjoining magnetic fields, the currents which circulate round two consecutive coils are always in opposite directions.

The ends of the coils are so connected up that the current circulates in each coil in the opposite direction from that in the preceding coil, and thus the sum of the force of the currents entering each coil at the same time is obtained.

In the above figure, as well as in those which follow, for the sake of clearness, the coils which are connected up in the circuit in the opposite way to the direction in which they are wound are shown in black, the others in white, and the magnetic fields of opposite polarity are distinguished by the black and white rectangles. It is to be remarked that coils which approach magnetic fields of similar colour induce currents in the direction of rotation of the machine, while coils which approach magnetic fields of dissimilar colour induce currents in the opposite direction. It will be seen that at the moment represented in the figure the coils are approaching magnetic fields of the same colour, whilst in an eighth of a revolution more the reverse will take place, so that, as already explained, the power of all the coils is united to produce a positive or negative current, while by the continued revolution of the machine strong currents will be induced, rapidly following each other in opposite directions, and constituting an alternating current.

The new dynamo machine has nearly the same external appearance as the alternate-current machine. The essential difference is that in the new machine

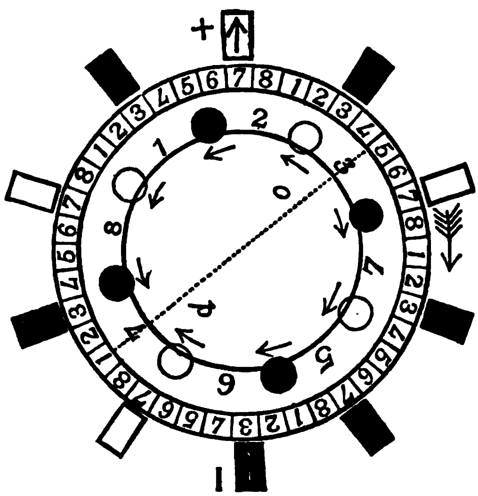

FIG. 3.

the number of induction coils is not equal to the number of pairs of electro-magnets. The number of each can be varied at pleasure, but we will suppose that in the present instance there are 10 magnetic fields and 8 coils, as shown in Fig. 3.

Since the magnetic fields are at a different distance apart from the coils, only two of the latter, diametrically opposite to each other, can be simultaneously in the magnetic fields, the other coils being at a greater or less distance from the adjoining fields.

When the coils rotate, therefore, the maxima of the current do not, as in the alternating machine, enter similar coils simultaneously, but enter successive coils at successive moments.

In order to unite all these induced currents into one continuous current, the ends of the armature-coils are so connected up that the wire on all the coils forms a continuous closed circuit, and is wound round two adjoining coils in opposite directions. Thus the induced currents which are produced in two consecutive coils reinforce each other as the coils approach simultaneously the corresponding magnetic fields, which of course have different polarities, as seen in Fig. 3. The axle of the machine carries a commutator, which consists of 40 plates insulated from each other and from the axle. The commutator plates are connected in 8 groups in such a way that each group includes 5 plates, each of which is separated from any other plate of the same group by the 7 plates of the other groups. These connections are made by means of 8 metal rings attached to the axle, but insulated from it, from each of which five wires lead to the commutator plates of the respective groups. From the connecting wire between each two consecutive coils a wire goes to each one of the groups of the commutator, and in such a way that two consecutive connections between the coils are connected to two consecutive plates of the commutator.

The current is taken off at two diametrically opposite points of the commutator, by means of springs or brushes.

The working of the machine will be understood by reference to Fig. 3, in which, besides the coils and magnetic fields, the commutator with its groups of plates is indicated by the outer numbered circle, the position of the brushes by the signs + and —, and the connecting wires between the coils by the lines numbered 1 to 8. For the sake of clearness, the connections between the coils and the plates have been left out, but it must be remembered that *each* of the wires numbered 1 to 8 is connected to *each* of the five plates which bears the corresponding number.

Imagine now that the coils, together with the commutator, rotate in the direction shown by the arrow, viz., with the hands of a watch, then no matter at what moment we consider the position of the coils with respect to the magnetic fields, we shall find that we can always draw a straight line through the centre of the diagram, which will divide it into two halves, in one of which like-coloured coils and fields approach each other, and in the other half unlike-coloured. This line is shown dotted in the figure.

All the coils in the one half give a positive current in the direction of rotation, and the coils in the other half in the contrary direction; accordingly from both halves positive electricity is led to the point 7, and negitive electricity to the point 3. It will also be seen from the figure that the plates 7 and 3 of the commutator, which are in connection with connecting wires 7 and 3 of

the coils, are under the brushes on either side, and therefore a current will flow through the external circuit of the machine.

In a similar manner it will be found that for any other position of the machine, the imaginary line of division always cuts the two points of the inner circle in connection with the similarly numbered plates of the commutator, which are for that position in contact with the brushes, whence it follows that from the + brush a continuous positive current is obtained, and a continuous negative current from the brush marked —.

The new continuous-current machine and the older dynamo machines have this point in common, that in the two halves the current flows in parallel shunt circuits. While in the latter, however, the two shunt circuits maintain their position, in the new machine they revolve in an opposite direction to, and much faster than, the axle, but on account of the peculiar arrangement of the commutator plates the ends of both circuits are always in contact with the brushes.

The general arrangement of such a machine is as follows, it being understood that the number of coils must not be equal to the number of magnetic fields:—Let n be an even number of rotating coils, and $n + 2$ the number of pairs of electro-magnets, the commutator should have $n\left(\frac{n}{2} + 1\right)$ parts. Between the windings of every two coils there is a connection with $\frac{n}{2} + 1$ parts of the commutator, which are at equal distances from each other. The current is always taken off at two diametrically opposed points of the commutator.

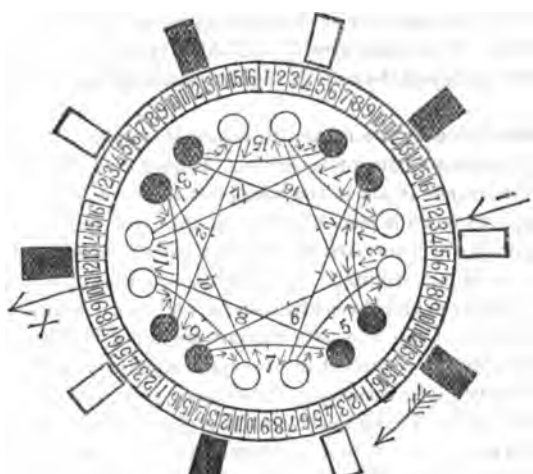

FIG. 4.

Instead of there being more magnetic fields than coils, there may be fewer, as is seen by changing the + sign into — in the above quantities, and the difference need not be always equal to 2. For instance, while retaining 10

magnetic fields, the number of coils may be doubled, *i.e.*, there may be 16 coils. This gives an advantage in that the sparking at the commutator is less, and the machine works better and more steadily. Fig. 4 shows a diagram of such a machine.

The coils are arranged in two series, so that one coil half covers the end of the opposite coil in the second series. It will be seen from the figure that the coils into which the currents enter in immediate succession do not follow each other, but are arranged in a zigzag manner along a circle, and are connected consecutively in the circuit of the current. On this account, the connections between the coils, which in Fig. 3 appeared as a circle, are here shown as lines crossing the circle backwards and forwards. In this case the commutator has 80 parts.

The author claims the following advantages for his new machine. The electro-magnets are directly excited by the continuous current circulating in their coils, while in former continuous-current machines the more important pole is magnetised by induction, and therefore with less strength. The rotating coils contain no iron, and no reversal of the magnetic poles occurs in the whole machine, and thus the loss of power, due to heating of the iron by such reversals, is avoided. In the existing machines of Gramme or Siemens, this heating does not reach a large amount, since the poles are only reversed twice in each revolution; but in the machine here described, the reversals, supposing an iron core to be used, as in the Alliance machine, would be much more frequent, and the heating would be considerable, entailing a greater expenditure of power to drive the machine. Another advantage lies in the simple manner of winding the coils, and in the possibility of thoroughly insulating the wire from the body of the machine. The armatures of the older types of machine had to be wound by hand, whereas the coils of the new machine can be readily wound in a lathe.

The author proceeds to compare the respective merits of continuous-current machines and alternate-current machines. The amount of light given by the two kinds of machines is about the same, but in the continuous-current one, since the positive carbon is placed at the top, the light is thrown more downwards, while with alternate-current machines each carbon is equally hot, and the light is more equally diffused. The alternate current gives the best light in a horizontal direction, and the continuous current in a downward direction. A further advantage of the alternate current machines is that, as the electro-magnets are excited by a current from a small dynamo machine, they are not so sensitive to any slight variations of resistance in the circuit, and they thus give a steadier light. This method of exciting the electro-magnets by a current from a primary machine can also be adopted very successfully with continuous current machines. Also the alternate-current machines do not require any commutator. This is the most critical part of a dynamo machine, both on account of the sparking, which cannot be entirely prevented, and because any faulty insulation may lead to great damage to the machine. With alternate-current machines, again, several independent circuits can be derived from the

same machine, so that the lamps may be easily arranged in groups, any one of which may be extinguished without affecting the others. Neither machine has a decided superiority, but each has its special uses. Both kinds can be used to work differential lamps, it being only necessary to proportion the thickness of the wire on the armature to the power required for the lamps. Whilst, therefore, both kinds of machines can be suitably employed for lighting, the continuous-current machines are alone available for electro-plating, refining of metals by electrolysis, or for transmission of power.

The author concluded by observing that the machine was still too new to admit of a full comparison with the older types, but he thought that, by uniting the currents arising from different poles of the machine, a new principle had been introduced, which would prove of importance in the further development of dynamo-electric machines.

INDEX OF VOL. X.

1881.

PAGE

ABSTRACTS.

BRITISH DELEGATES TO THE ELECTRICAL CONGRESS.

(Up to date of publication.)

CRAWFORD AND BALCARRES, The Earl of, F.R.S.,

Chief Commissioner.

Abel, Prof. F. A., F.R.S.
Adams, Prof. W. G., F.R.S.
Ayrton, Prof. W. E., F.R.S
Barrett, Prof. W. F., F.R.S.E.
*Bright, Sir Charles, M.I.C.E.
Crookes, W., F.R.S.
Clark, Latimer, M.I.C.E.
Clifton, Prof. R. B., F.R.S.
Chrystal, Prof. G., F.R.S.E.
De La Rue, Dr. Warren, F.R.S.
Everett, Prof. J. D., F.R.S.
Fitzgerald, Prof. G. F., F.R.I.A.
Foster, Prof. G. C., F.R.S.
Gordon, J. E. H., B.A.
Gladstone, Dr. J. H., F.R.S.
Graves, E.
*Hughes, Prof. D.E., F.R.S.
Hopkinson, Dr. J., F.R.S.
Jenkin, Fleeming, Prof., F.R.S.
Moulton, J. F., F.R.S.
Preece, W. H., F.R.S.
Smith, Willoughby.
Siemens, Dr. C. W., F.R.S.
Spottiswoode, W., Pres. R. S.
Stroh, A.
Spagnoletti, C. E., M.I.C.E.
Thomson, Sir W., F.R.S.
Varley, Cromwell, F.R.S.
Walker, C. V., F.R.S.
*Webber, Lieut.-Col., C. E., R.E.

*** Commissioner for the Electrical Exhibition.**

Honorary Secretaries to the Commission:

In Paris—Aylmer, J., C.E.

In London—Webb, F. H.

General Classification of Objects in the Exhibition.

GROUP I.—PRODUCTION OF ELECTRICITY.

Class 1.—Statical Electricity.

Class 2.—Batteries and Accessories.

Class 3.—Magneto and Dynamo Electric Machines.

GROUP II.—TRANSMISSION OF ELECTRICITY.

Class 4.—Cables, Wire, Accessories, and Lightning Protectors.

GROUP III.—ELECTROMETRY.

Class 5.—Apparatus used in Electrical Measurements.

GROUP IV.—APPLICATIONS OF ELECTRICITY.

Class 6.—Telegraphic Signalling.

Class 7.—Telephonic, Microphonic, and Photophonic Apparatus.

Class 8.—Electric Light.

Class 9.—Electric Motors used in the Transmission of Power.

Class 10.—Medical Electricity.

Class 11.—Electro-Chemical Apparatus.

Class 12.—Instruments for Measurement, Electro-Magnets, Compasses.

Class 13.—Various Apparatus.

GROUP V.—GENERAL MACHINERY.

Class 14.—Steam, Gas, and Hydraulic Motors and suitable Transmitters for Electric Industries.

GROUP VI.—BIBLIOGRAPHY, HISTORY.

Class 15.—Bibliographical Collections of Scientific Works, and of those referring to Electrical Industries, Plans, Maps, &c.

Class 16.—Historical Collections of Apparatus used in the early investigations and in the oldest applications of Electricity.

Explanation of Reference Numbers in this Guide Book.

"*1* and *3*. Gr. i., cl. 3, **1444**—Gr. iv., cl. 8, **1502**—Gr. iv., cl. 11, **1521**.

ANGLO-AMERICAN BRUSH ELECTRIC LIGHT CORPORATION, LIMITED,

74, *Hatton Garden, London, E.C.*"

Means—First, that the Brush Company's exhibits occupy stalls numbered *1* and *3* in this Guide Book, and may be seen in this order by a visitor following the route indicated in this book; secondly, that they are classified under group i., class 3, in the French Official Catalogue, and bear the number 1444 in that group and class; that they are also classified under group iv., class 8, bearing the number 1502 in that group and class; and again under group iv., class 11, where they bear the number 1521.

BRITISH MACHINERY SECTION.

South East of the Electric Railway Station.

The numbers in this Guide Book commence at the East end of the British Machinery Section.

1 and *3*. Gr. i., cl. 3, **1444**—Gr. iv., cl. 8, **1502**—Gr. iv., cl. 11, **1521**.

ANGLO-AMERICAN BRUSH ELECTRIC LIGHT CORPORATION, LIMITED.

74, Hatton Garden, London, E.C.

One dynamo-electric machine, capable of working 40 = 2,000 candle power arc lamps.

One	ditto	16 = 2,000 ditto.		
One	ditto	a single light of 6,000 candle power.		

One dynamo-electric machine, capable of working 3 lights of 2,000 candle power each.

One	ditto	4	lights of	3,000.
One	ditto	2	„	2,000.
One	ditto	1	„	150,000.
One	ditto	1	„	50,000.
One	ditto	1	„	3,000.
One	ditto	1	„	1,500.

One plating machine and about 100 assorted arc lamps.

There will also be exhibited a dynamo machine, driving about 200 Lane Fox incandescent lamps. In addition to the above there will be a collection of various details of lamps, carbons, wires, &c.

2. Gr. iv., cl. 8, 1515.

J. W. SWAN,

Newcastle-on-Tyne.

INCANDESCENCE LAMPS OF VARIOUS FORMS AND WITH VARIOUSLY ARRANGED FITTINGS.

Swan's Lamp consists of a bulb of glass (from which air has been almost completely exhausted) containing a thin filament of carbon, which, when an electric current passes through it, becomes incandescent.

This exhibit comprises chandelier, bracket, and table lamps.

Among the special adaptations of this lamp, Mr. Swan exhibits:—

The miner's lamp, of various designs.

The diver's lamp.

Workshop lamps.

A group of lamps, arranged for signalling, &c.

3, as above.

2 and *3.* Gr. v., cl. 14.

ROBEY & CO.,

Lincoln and 117, *Cannon Street, London, E.C.*

PATENT ROBEY FIXED ENGINE & LOCOMOTIVE BOILER COMBINED.

One 20-h.p. patent "Robey" electric light, fixed engine.			Driving the machines of Messrs. Brush at stands No. 1 and 3, and of Mr. Swan, stand No. 2.
One 40-h.p.	ditto	ditto.	
One 25-h.p.	ditto	ditto.	
One 20-h.p.	ditto	ditto.	
One 16-h.p.	ditto	ditto.	
One 12-h.p.	ditto	ditto.	
One 12-h.p.	ditto	ditto.	
One 14-h.p.	ditto	ditto.	

This engine is specially designed for providing economical steam power in a small space. We would direct attention to the way in which the boiler is connected to the engine by being bolted to the cylinder only, and carried by rollers working in grooves at firebox, and thus relieving the boiler of all strain. The base-plate is formed at one end into an ash

pit with damper doors, and is made suitable for receiving the firebox end of the boiler, the other end of which is carried by a crutch-shaped casting fixed over the cylinders. The end of the base-plate under the cylinders is formed into a feed water-heater tank into which the cylinder cock discharges all condensed water, and into which a portion of the exhaust is so directed as to heat the feed water to nearly boiling point before going into the boiler. The whole of the parts of both engine and boiler being included on one foundation or base-plate, heavy and expensive foundations are dispensed with, the weight of the boiler and its contained water acting as an extra weight to assist in keeping the whole machinery in rigid position. All engines are fitted with patent equilibrium governors, which are admirably adapted for working in connection with electric light apparatus.

4. Gr. iv., cl. 9, **1517.**

LATIMER CLARK, MUIRHEAD & CO.,

Regency Street, Westminster, London.

Dr. Hopkinson's electro-magneto machine for continuous currents.
Ditto ditto alternate currents.

5. Gr. iv., cl. 8, **1509.**

ROWATT & FYFE'S ELECTRIC CO., LIMITED,

52, Queen Victoria Street, London, E.C.

Schuckert's dynamo machine.
Two Siemens' D.2 machines.

6. Gr. i., cl. 3, **1448**—Gr. v., cl. 14, **1538.**

R. E. CROMPTON & CO.,

Arc Works, Chelmsford.

The main exhibit is a compound semi-portable steam engine, constructed by Messrs. Marshall, Sons, & Co., of Gainsborough, to specification of exhibitors.

In this engine the desire has been to produce a design free from complication and to obtain great regularity of strain on the crank shaft throughout each revolution, and at the same time get the advantage of using high grades of expansion.

There are two cylinders, 7 and 11½ dia., both thoroughly steam jacketted. The high pressure cylinder has a slide valve, the cut off of which is controlled by a Hartnell governor, similar to those used on their simple engines in all their permanent installations.

The engine can be started when all circuits are closed (and consequently considerable power is required) by opening a small valve and thus admitting the steam direct from the boiler to both cylinders; after the engine gets well under way this valve is closed and the engine works compound. With this engine at Paris it is expected to work twelve lights on four Bürgin Machines, with a consumption of 40 lbs. of Welsh coal per hour, in other words the exhibitors hope to obtain a light of 1500 candles per hour for expenditure of each lb. of fuel. They are showing *four Bürgin Dynamo-Electric Machines.* The first two are of their ordinary pattern, the armature being wound with .057 wire. The magnets .141 wire. The total internal resistance of he machine being 2.16 ohms. The E. M. F. at 1550 revs. per minute is 200 volts. Three arc lights, nominally 400 candles each, can be burned in series.

The third machine is wound with same wire on magnets, but .065 wire on armature. Its E. M. F. is 160 volts. Two very large arc lights, 50 per cent. larger than those named above, can be burned in series, or sixty Swan lamps can be burned in three series of twenty each.

The fourth machine is wound with similar wire to the first named, but has ten rings instead of eight, and consequently has a collecting cylinder with sixty divisions. This machine drives from four to six arc lights of equal power to those first named. The E. M. F. is 300 volts.

7. Gr. i., cl. 3, **1446**.

BRITISH ELECTRIC LIGHT COMPANY,

Heddon Street, Regent Street, London.

Gramme machines of various new types, giving variable currents according to requirements.

Various apparatus for use with above.

8. Gr. v., cl. 14.

THE DOWSON ECONOMIC GAS CO., LIMITED,

3, *Great Queen Street, Westminster, London.*

This exhibit represents an apparatus for making cheap gas suitable for gas engines, which can drive dynamo and other machines. After several

trials with some of Messrs. Crossley's 3½-h.p. (nominal) "Otto" gas engines, it has been proved that by using this gas the actual consumption of coal, after allowing 10 per cent. for waste and impurities, is 1.4 lbs. per indicated h.p. per hour. The total cost of this gas, including wages, &c., and when made on a comparatively small scale, is under one shilling for the equivalent of 1000 c.f. of ordinary coal gas which usually costs 3*s.* to 4*s.* The cost can be still further reduced when the gas is made on a large scale. This gas has been in use over two years for several manufacturing processes, and the above figures as to cost have been fully confirmed. The exhibitors shew also models of their apparatus in the General British Section. The inventor is Mr. J. Emerson Dowson, C.E.

9. Gr. i., cl. 3, **1449.**

W. T. HENLEY,

North Woolwich, London.

One vertical dynamo electric machine.

Two horizontal dynamo electric machines.

10. Gr. v., cl. 14, **1544.**

WALLIS & STEVENS,

North Hants Iron Works, Basingstoke.

A semi-fixed steam engine, with patent governor gear for adjusting the speed while in motion to any desired point. An invaluable addition, as the speed required to give the best light varies constantly with the state of the atmosphere and the adhesion of the belts.

11. Gr. v., cl. 14, **1540.**

RANSOMES, HEAD, & JEFFERIES,

9, Gracechurch Street, London, and Ipswich.

Messrs. Ransomes, Head, & Jefferies, of the Orwell Works, Ipswich, and 9, Gracechurch Street, London, who have devoted considerable attention to the manufacture of steam engines specially adapted for driving electric light machines, exhibit the following, namely :—

1. 10-*h.p. Semi-Portable Engine,* with multitubular boiler, patent automatic governor expansion gear, with all usual fittings, specially adapted for driving dynamo-electric machines. This engine is of the same type as the one employed for driving the electric light on the Thames Embankment, London.

1. *H size,* 6-*h.p. Vertical Engine,* with patent automatic governor, expansion gear, and usual fittings. This type of engine is used on board

steamers, and is arranged to drive he dynamo machine by gearing instead of belting, although a plain flywheel can be used when preferred.

12. Gr. v., cl. 14, **1543.**

THOMSON, STERNE & CO., LIMITED,

The Crown Iron Works, Glasgow;
10, *Victoria Chambers, Westminster, London, S.W.; and*
10, *Rue Laffitte, Paris.*

CLERK'S PATENT GAS ENGINE.

In Clerk's patent gas engine an ignition is made at every revolution instead of one ignition at every two or more revolutions, as is usual in existing gas engines; the power is therefore much greater for the size of the engine. By a special and simple arrangement all tendency to back ignition is avoided, and the engine may be worked up to its full available power without a tendency to irregularity. It is much steadier than ordinary gas engines, and therefore better suited for *dynamo electric* purposes. It is extremely simple, and has no gearing wheels of any kind. The pressure necessary on the ignition slide is very low, as the ignition port is small. There is thus no liability of the valve to cut up or possibility of absorbing power.

14. Gr. v., cl. 14, **1537.**

PETER BROTHERHOOD,

56, Compton Street, London, E.C., and 10, *Rue Laffitte, Paris.*

One Brotherhood 3-eye engine, driving a No. 5 Brush dynamo-electric machine.

One Brotherhood 3-eye engine, driving a pair of Dr. Siemens' dynamo-electric machines.

One Brotherhood 3-eye engine, driving a pair of C. T. Gramme dynamo-electric machines.

15. Gr. v., cl. 14.

A. FIDDES,

Lewin's Mead, Bristol.

The "Fiddes" silent gas engine, in motion. By means of an entirely new action in gas engines, the "Fiddes" gas engine is rendered very powerful and simple in construction, as well as using less gas than most other gas engines.

GENERAL BRITISH SECTION.

North West of the Electric Railway Station.

The numbers in this Guide Book commence at the North East end of the General British Section.

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1. OFFICE OF THE ROYAL BRITISH COMMISSION.

2 and *3*. Gr. ii., cl. 4, **1461**—Gr. iv., cl. 6, **1491**—Gr. iv., cl. 8, **1514**.

MESSRS. SIEMENS BROTHERS & CO.,

Charlton, near Woolwich.

Nine battery cells of various patterns and sizes.

Three battery boxes.

Six Siemens' patent dynamo machines, D^3 upright in operation, excited by one D^7 dynamo machine.

One Siemens' patent alternate current machine, W^2 in operation, excited by a D^5 dynamo machine.

One Siemens' patent W^1 alternate machine, arranged to burn 28 lights in 4 circuits of 7 lights each.

One Siemens' patent D^3 dynamo machine upright.

One	ditto	D^7	ditto.	
One	ditto	D^6	ditto.	
One	ditto	D^5	ditto.	
One	ditto	D^6	ditto,	having its electro-magnets

connected parallel with the external circuit.

One Siemens' *large* dynamo-electric quantity exploders. Fuses ¼-inch length of iridio-platinum wire 0.0015-inch diameter, through 70 units.

One Siemens' *small* ditto. Fuses ½-inch of same size iridio-platinum wire, through 50 units.

One Siemens' magneto-electric quantity exploder, with 9 magnets.

One ditto ditto, with 6 magnets.

One ditto ditto ditto, cheap form.

One ditto large dynamo-electric tension exploder. Length of spark $\frac{200}{3000}$ inch.

One ditto small ditto. Length of spark $\frac{130}{3000}$ inch.

One Siemens' magneto-electric tension exploder, with 9 magnets. Length of spark $\frac{80}{3000}$ inch.

One Siemens' magneto-electric tension exploder, with 6 magnets. Length of spark $\frac{18}{3000}$ inch.

One Siemens' magneto-electric tension exploder, with 6 magnets, cheaper form. Length of spark $\frac{10}{3000}$ inch.

One Siemens' No. 10 iron tubular telegraph pole, with 4 stretching insulators.

One Siemens' No. 5 iron tubular telegraph pole, with buckled plate and 4 intermediate insulators.

One Siemens' No. 105 iron tubular telegraph pole, with Le Grand and Sutcliff's patent base-pile.

One Siemens' No. 110 iron tubular telegraph pole, with Le Grand and Sutcliff's patent base-pile.

Two rammers for driving piles.

One short wooden pole, with 17 insulators of various patterns.

One short wooden pole, with 22 insulators of various patterns.

One short wrought iron pole, with 11 insulators of various patterns.

One short wrought iron pole, with 10 insulators of various patterns.

One small box, with samples of Siemens' compound wire.

Four small coils of Siemens' compound wire.

Three insulators for electric light leading wire.

Samples of leading wire for electric light use.

Samples of underground cable for electric light use.

One dial pattern testing bridge.

One portable testing bridge.

One set of testing apparatus, on ebonite pillars.

One Thomson's reflecting galvanometer.

One portable folding lamp and scale stand for same.

One Jacob's transparent ditto.

One Siemens' universal galvanometer.

One Obach's galvanometer for measuring powerful currents.

One Siemens' electro-dynamometer for measuring powerful currents.

One Siemens' electro-dynamometer for measuring small currents.

One duplex condenser from ½ to 2 m.f. capacity.

One condenser of 0.37 m.f. capacity.

One Siemens' electrical pyrometer, complete.

One box of comparison resistance coils, 10,000 ohms.

One box of branch resistance coils, from 2 to 1000 ohms.

One carbon resistance frame, 3 units.

One hoop-iron resistance frame, 0.1 to 1 unit.

One set of portable field outpost telegraph apparatus.

Two intermediate Morse double-current duplex translation instruments.

Two Morse double-current self-starting end local instruments.

One model A B C instrument, with finger key.

One round ditto ditto ditto handle.

One square ditto ditto ditto.

Six Siemens' hanging pendulum lamps, burning in connection with the 6 D^2 dynamo machines.

Eight Siemens' differential lamps, burning in connection with the W^3 D^5 machines.

One Siemens' hanging pendulum lamp, with abutment pole, City of London pattern, with reflector.

One Siemens' hanging pendulum lamp, enclosed in square lantern, with reflector.

One Siemens' hanging pendulum lamp, with cover removed

One Siemens' large pendulum lamp, upright.

One ditto ditto, with abutment pole in parabolic reflector.

One Siemens' small pendulum lamp, upright.

One ditto ditto, with abutment pole.

One ditto ditto, in reflector, upon cast iron stand, with universal motion.

One Siemens' duplex hanging pendulum lamp on stand.

Two Siemens' differential lamps, with abutment poles on lamp posts, City of London pattern.

One Siemens' differential lamp, with cover removed.

One ditto ditto, for ships use.

One ditto ditto, with cam adjustment.

One ditto medium focus-keeping clockwork lamp, with reflector and focus observer.

One Siemens' small focus-keeping clockwork lamp in Fresnel reflector, upon cast iron stand, with universal motion.

One Siemens' small focus-keeping lamp, as above, with cover removed.

One Siemens' lamp, with horizontal carbons.

One globular lantern, with pendant, ash tray, counterweight, and 20-inch alabaster globe.

One globular lantern, with reflector, City of London pattern.

One square lantern, with inverted umbrella-shaped reflector and weather covers.

Samples of carbon rods for electric light.

One single-leaf horse-shoe magnet, weighing 1 lb. 9¾ ozs., and capable of supporting 3½ lbs.

One five-leaf horse-shoe magnet, weighing 7 lbs. 15 ozs., and capable of supporting 84 lbs.

One micrometer gauge, graduated inch and millimeter.

One „ „ inch.

4. MESSRS. SIEMENS' SUBMARINE TELEGRAPH TROPHY.

Centre of East end of General British Section.

One submarine trophy, consisting of—

A telegraph buoy, completely rigged with flagstaff, lamps, and bridlechain.

A coil of 6 by 4 steel and hemp grappling rope, one end shackled on to the bridlechain of buoy, and the other to a mushroom anchor.

A 2½-cwt. ordinary 5-prong grapnel.

A set of connections for steel and hemp grappling rope.

A model of their telegraph steamer "Faraday."

Three stones picked up from the bed of the North Atlantic Ocean.

Specimens, illustrating manufacture of gutta percha and guttta percha core.

Specimens, illustrating manufacture of India rubber and India rubber core.

Cases containing specimens of submarine, subterranean, military, and torpedo cables.

Platino Braziliera cable, 1873.
Direct United States cable, 1874.
French Atlantic cable, 1879.
New American Atlantic cables.

Table to the South of the preceding Trophy.

5. Gr. iv., cl. 6, **1490.**

MESSRS. SAXBY & FARMER,

Patent Railway Signal Works, Canterbury Road, London, N.W.

Full-size machine, illustrating "The mechanical union of the block and interlocking systems." The apparatus consists of seven levers for working points and signals, and the necessary telegraphic block signalling instruments for working between three consecutive stations on a railway. The important feature of this invention is that it combines in one apparatus the levers for working the points and signals, and the handles of the train telegraph instruments, all being reciprocally interlocked, so that they cannot be manipulated in a contradictory manner.

Some of the special advantages are that the points and out-door signals must be in proper position before "line clear" can be telegraphed, and no points can be improperly shifted after the line has been signalled "clear." Having signalled "line clear" once it is impossible to do so a second time until the first train has passed out of the section. All points which have been locked by the action of giving "line clear" remain locked till the expected train has passed through the station.

The out-door starting signal is compelled to be re-set to danger behind every train, and it is impossible to signal a train to enter a block section without first announcing it to the next station on the telegraph. The starting signal to permit entrance into a block section cannot be given without the consent and concurrent action of the signalman at both ends of the section.

Table to the South West of Messrs. Siemens' Trophy.
South East end of Table.

6. Gr. iv., cl. 6, **1481.**

W. M. FOXCROFT,

54, Compton Street, Clerkenwell, London, E.C.

Comprises block instrument cases, single needle, North Eastern Railway pattern; speaking instrument cases, single needle, Midland Railway pattern, speaking instrument cases, G.P.O. pattern for commutator instrument, also Gower bell telephone cases, as adopted by Her Majesty's Government, or G.P.O.; also special telephone and battery cases combined. Being portable they can be used for men of war, torpedo and field purposes. Each division, boat, &c., being supplied with one by the paying out of a wire, communication is easily maintained. Transmitter and receiver case.

7. Gr. i., cl. 2, **1436**—Gr. ii., cl. 4, **1452.**

JOSEPH BOURNE & SON,

Denby Pottery, near Derby.

SPECIMENS OF STONEWARE INSULATORS, BATTERY JARS, &c.

The peculiar qualities resulting from the composition of the paste from which these insulators are produced are a high degree of vitrefaction, combined with great toughness and strength. These qualities have led, since the commencement of the manufacture, thirty years ago, to a large and increasing demand. For many years the firm enjoyed the exclusive right to manufacture the well-known double V insulators, patented by Mr. Varley, whilst these and other shapes—notably the Z pattern—have been largely supplied to the English Government Telegraph Department. Specimens of these are exhibited.

The larger exhibits are copies of the designs of Colonial and Foreign Engineers, who look for strength and durability, as well as perfect insulation for long distances, whilst the smaller specimens have been found equally adapted to telephone and mining purposes.

The same qualities enter into the composition of the battery cells exhibited.

8. Gr. iv., cl. 13, **1534.**

WEBB BROWNE WOLSEY,

22, Harrington Square, London, N.W.

Electric Automatic Tell Tale Till, three specimens of an, arranged for English, French, and American currency.

The object of this invention is for the protection of retail traders from the pilfering of employés entrusted to take cash. It is quite novel in its operation and construction, and is patented in Great Britain, America, and other countries. The mechanism is contained in a small box, on the top of which are several dials for the £ *s. d.*, &c. On these are registered not only each separate amount paid in, but they show at a glance the total of the money that has been received during the day; moreover, at each operation it rings out on different bells the various amounts paid by the customer, either in front of the payer or in any other convenient place. It cannot be tampered with and is adapted to any kind of trade, especially for very quick businesses. It requires but little attention, as it is worked by a Leclanché battery (that requires charging four times during the year), and is not very liable to get out of order.

9. Gr. ii., cl. 4, **1453**—Gr. iv., cl. 12, **1525.**

MESSRS. W. T. HENLEY,

North Woolwich.

A Case of Samples of Cables manufactured by Messrs. W. T. Henley up to 1879.

A Permanent Magnet.

10. Gr. v. cl. 14, **1542.**

FREDERICK J. SMITH,

Taunton.

DYNAMOMETER (MODEL).

The dynamometer is designed to show at any instant the amount of force transmitted in working any machine under trial.

Description: two bevil wheels, one receiving motion from a prime

mover, the other transmitting the motion to the machine to be tested, are in gear with a third bevil wheel between them, the axis of which is at right angles with the axis of the former wheels, and capable of large angular displacement; this displacement, which is controlled by a spiral spring, is drawn on a drum carrying paper, which moves at a certain ratio to the belt speed. The diagram, as in other cases, shows the force transmitted. Another tracer, actuated by an electro-magnet, controlled by a seconds pendulum, indicates on the same paper the seconds during which any particular area of diagram has been traced. The counter shows the number of revolutions for rough approximation.

The dynamometer exhibited is intended for light laboratory work, and has been used in connection with the whirling table, and in electric and heat questions.

11. Gr. iv., cl. 7, **1493**—Gr. iv., cl. 12, **1524**—Gr. vi., cl. 16, 1551.

CONRAD W. COOKE, C.E., M. Soc., T.E., and E.,

5, Westminster Chambers, Victoria Street, London.

(1) TELEPHONIC ALARUM.

This instrument consists of a brass wheel centred to an upright bracket attached to a stand, and capable of being rotated in a vertical plane by a small winch handle projecting from its face. The edge of this wheel, which is about 2½ inches in diameter, is milled, and against the milled edge presses the point of a light metallic spring, so placed that when the wheel is turned the point of the spring rises and falls as the teeth of the milling pass beneath it; and, both being included in the circuit of a battery and telephone, a vibratory or intermittent current is produced when the wheel is turned. In order to prevent the battery being by mistake left in connexion with the telephone line, whereby it would be running to waste and injury be done to the telephones in circuit, the instrument is provided with an ordinary electric bell-push or transmitting key attached to the stand of the instrument, and the connexions with the attachment terminal screws and the spring and wheel are so arranged that the battery and wheel are cut out of the circuit altogether, unless the button is pressed as in the act of ringing an electric bell. In order to call attention at a distant station all that is necessary is to press down the button with the left hand, and to give a turn to the wheel with the right.

(2) GALVANOMETRIC BATTERY FOR DEMONSTRATING THE INTERNAL CURRENT TRANSMITTED THROUGH THE LIQUID WITHIN A VOLTAIC CELL.

This instrument consists of two glass cells united together by a small tube about two feet long, and convoluted into two circular coils after the manner of a Thomson's Reflecting Galvanometer. Within the coils is suspended an astatic system of magnetic needles, to which is attached a light mirror, by which its deflections may be made apparent by the movement of a spot of light on a screen. It may therefore in this respect be looked upon as a Thomson's Reflecting Galvanometer, with coiled liquid instead of with metallic wires. The elements are placed one in each of the little cells, and may be connected by a key; or, by placing a reflecting galvanometer in the external circuit, both currents may be simultaneously indicated on the screen and their interdependence or identity be demonstrated. Below the base of the instrument is a fine slightly magnetised sewing needle, which can be rotated on a vertical axis through a small angle by means of a little lever, and by which the instrument may be abjusted to zero.

(3) A modified form of the last instrument, in which each of the four glass coils constitutes part of a separate circuit between the two glass coils containing the elements, each being coiled in such a direction with respect to the magnetic needles, that the dynamic effect of all four currents upon the astatic system is the same in direction. By this arrangement the total resistance is reduced to the proportion of almost 16 to 1, and a very much more sensitive instrument is obtained. The glass work of this galvanometer, which is a marvel of difficult glass blowing—the two cells and four coiled tubes being all in one piece—was made for and presented to the exhibitor by Mr. Charles H. Gimingham.

Mr. Conrad W. Cooke also exhibits specimens of early forms of telegraph keys; an ivory pocket compass and sun dial 265 years old; an old compass from China, and one of the late Sir Charles Wheatstone's polarised relays.

12. Gr. iv., cl. 7, **1495**.

THE GOWER-BELL TELEPHONE CO., LIMITED.

9, *Great Winchester Street, London.*

Gower-Bell loud-speaking telephones, being the form of instrument adopted by the British Government Telegraph authorities.

13. Gr. iii., cl. 5, **1473.**

MAX SABEL,

2, Coleman Street Buildings, Moorgate Street.

a. Rolls of Telegraph Tape

For Wheatstone instrument, rapid automatic writer.

For Sir W. Thomson's recording instrument, submarine cable.

For Cowper's telegraph writer.

For the Morse, Hughes & Meyer systems, &c., &c.

For automatic machines.

b. Rolls of Ribbon-paper, or winding-tapes.

c. Rolls of Tape for railway metals, testing machines, train velocity gauges, and all other technical purposes, according to instructions.

The rolls are made in all sizes, to fit exactly the respective machines and the paper is tinted any shade required: the paper will possess all the properties which are required. The strips are rolled tight on the drum, parallel along the whole length, cut sharp and clean, and are freed from dust by a patent process.

The dust-free glazed telegraph paper is also shown, and is much superior to the ordinary unglazed paper, since with it any check in running off the drum is rendered absolutely impossible, the type shows up sharp and clearly defined, and the ink is absorbed in the shortest possible time.

14.

W. M. FOXCROFT,

54, Compton Street, Clerkenwell, London, E.C.

Trophy of Telegraph Instrument Cases in the centre of preceding Table.

Table to the North of the preceding Table, that is North West of Messrs. Siemens's Central Trophy.

15. Gr. ii., cl. iv., **1460.**

JAMES E. & SAMUEL SPENCER (The Rustless and General Iron Co.),

3, Queen Street Place, Cannon Street, E.C. Works: West Bromwich.

Exhibit consists of all kinds of black, galvanized, enamelled, and anti-corrodo (coated by Barff's rustless process) Wrought Iron Tubes and

Fittings; also wrought and cast iron telegraph poles and fittings, cast iron pipes, plain and coated by Dr. Angus Smith's solution, insulators and the various accessories of telegraph, telephone, and electric light companies.

A service of these anti-corrodo tubes, which had lain for eighteen months in sandy soil, impregnated with salt water, in South America, were, on being recently uncovered, reported by the engineer who reported on them to be quite sound, and free from corrosion.

These anti-corrodo tubes and fittings are particularly applicable for telegraph poles and conveying wires underground, as, being incorrodible, they do not choke from internal corrosion nor require replacing at frequent intervals like ordinary tubes. Thus that great inconvenience to the public —the breaking up of roads—is done away with, except in the first instance for laying.

16. Gr. ii., cl. 4, **1459**.

REID BROTHERS,

12, Wharf Road, London, N.

Sole proprietors of Heaviside's patent wire for the prevention of induction in telepone circuits both in underground and suspended wires.

Sole licensees for Creighton's patent insulator for fixing wires without the use or aid of binding wire.

17.

DOWSON & CO.,

3, Great Queen Street, Westminster.

Model of Economic Gas Generating Apparatus, shown in the British Machinery Section.

18. Gr. iv., cl. 6, **1486**.

T. REID,

68, St. Andrew's Street, Dundee.

19. Gr. i., cl. 2, **1438**—Gr. iv., cl. 8, **1508**.

Mr. F. E. FAHRIG,

Southampton.

1. Samples of the finest English gas retort carbon cut by F. E. Fahrig's

improved machinery into diverse shapes for different purposes, varying from a large battery plate or block to small 1-inch by $\frac{1}{8}$ of an inch discs for telephonic purposes. This kind is superior to manufactured or compressed carbon, especially for battery plates, and excellent results are obtained with it when employed as electrodes in electric lighting, also as electrodes for medical purposes.

2. Crushed carbon for battery use.

3. Ditto ditto and manganese.

4. Finest quality of manganese in crystal form (benoxide of manganese).

5. Pure and finest carbon dust for electro, medical, or chemical purposes, containing no foreign matters suitable for analysis.

AUTOMATIC "TIME" REGULATORS.

An arrangement which consists of a self-winding regulator of Vienna form, and as many dials as are required; the whole is driven by a very constant gravity battery of F. E. F.'s construction, and lasts for two years, which is placed in the regulator *case* (bottom); it is a most reliable arrangement and without trouble whatsoever a *correct* time can be recorded in many places with our regulator and our battery only. Here the labour of winding or setting hands, or watching the driving clock is dispensed with; *once* started, the pendulum is self-acting and complete. The secret of its correctness lays in its simplicity. The regulators and diverse dials are beautifully designed, and are with or without illuminated dials.

20. Gr. ii., cl. 4, **1455**—Gr. iv., cl. 7, **1496**.

MESSRS. RICHARD JOHNSON & NEPHEW,

Bradford Iron Works, Manchester.

No. 00 B. W. G. Best Galvanized Iron Rod for Cable Armour. The special feature of this specimen is to show the great length that can be made in one piece without weld or joint, thus minimizing welding in cable making, and, therefore, less weak places. This coil weighs over 200 lbs.

No. 8 B. W. G. Galvanized Telegraph Wire of low electrical resistance. This meets in every respect the special requirements of the British Post Office Authorities, and their specification can be manufactured in lengths of single pieces, 140 lbs. each.

No. 16 by 3 Ply Galvanized Telegraph Wire Strand is also prepared to meet the British Post Office Authorities' specifications and requirements in every respect.

No. 11 Galvanized Special Telephone Wire is prepared expressly for telephone work, and can be made in single pieces up to 140 lbs. if required; and, from what has already been experienced in the erection of telephone lines, it has satisfactorily filled every requirement.

No. 15 Galvanized Special Telephone Wire of a high breaking strain. The same remarks are applicable to this as to the No. 11, with this exception: this is prepared for erection in towns and cities where appearance and light supports are wanted, and at the same time giving an increased strain as a margin of safety from lines breaking down and falling across streets.

21. MESSRS. RICHARD JOHNSON & NEPHEW.

Trophy of Telegraphing Wire in the centre of the preceding Table.

North of Messrs. Siemens' Central Trophy.

22. Gr. iv., cl. 8, **1507**.

R. E. CROMPTON & CO.,

Arc Works, Chelmsford.

Lamps.—These are showing twelve arc lamps in all, and eight miner's lanterns, fitted with Swan lamps, as used by the exhibitors at Pleasley Colleries.

Of the first-named, nine lamps are similar in type to those they are using at King's Cross Station and at Norwich.

This lamp has an extra magnet, wound with fine wire, which assists in relieving the brake-wheel from the pressure of the jockey armature.

They also show these lamps in lanterns, two lamps in each lantern, with an automatic switch, which switches in the spare lamp in case any failure occurs in the lamp first lighted.

Also a new pattern of arc lamp, having only one large worm wheel gearing into the rack.

They also show a new pattern double lamp, which carries a double set of carbons, which will run twenty hours without replacing carbons.

Also shown are some automatic cut-out switches for use with single lamp, to prevent the failure of any one lamp affecting others in same circuit.

Against the North wall and adjoining the office of the British Commission.

23. Gr. iv., cl. 6, 1489.

JULIUS SAX,

103, *Great Russell Street, Bloomsbury.*

PATENT MECHANICAL & ELECTRICAL WATER GAUGE.

By means of this apparatus the engineer at the Pumping Station is enabled to know the exact state of the cistern, tank, or reservoir at any time by merely looking at the electric dial, which may be fixed close to the engine.

The apparatus consists of two parts—

No. 1. The mechanical indicating water gauge, fixed at the top of the cistern.

No. 2. The electrical counterpart, which may be fixed at *any distance* away from the cistern.

The mechanical water gauge carries a metal float, which is so arranged that its rising and falling will cause the hand on the dial to move forward or backward, as the case may be, and if once set so as to point on the dial the height of the water in the cistern, the hand would always follow the same and indicate it.

The hand on the electrical dial is made to follow the movement of the hand on the mechanical dial by reversed currents. If the pointer of the electrical dial is set to correspond with the pointer of the mechanical dial, the former would always follow the latter, whether it went forward or backward, and so show the state of the cistern or reservoir at any distance away from it.

The apparatus is also arranged to ring an alarm bell, both when the water has reached either the highest or lowest point.

Also electric bells for offices, &c. Indicators for telephone stations, &c.

MR. JOHN W. FULLER'S PATENT "MIRROR" GALVANOMETER, for improving the working of long Submarine Telegraph Cables, &c. Also combined switch and commutator.

24. Gr. iv., cl. 10, **1518.**

B. COPSON GARRATT,

MEDICAL ELECTRICIAN,

15, *Finsbury Square, London, E.C.*

MAGNETO-ELECTRIC APPLIANCES.

These exhibits show the latest and most approved method of treating disease by means of aura or currents, discharged from permanent magnets, enclosed in various garments, and applied to the motor points and vital organs of the human frame.

The exhibitor claims that he has solved the problem of the adaptibility of magnetic treatment to intricate cases of disease by introducing compound magnets of various powers in different positions, according to the requirements of the patient. The extreme lightness and flexibilty of his special magnets, enabling him to concentrate a high degree of magnetic power at any desired point without discomfort or appreciable weight. It is also claimed that magnets prepared by this special process permanently maintain their therapeutic virtue.

For particulars of Mr. Garratt's method of treatment, see his various pamphlets on Nerve Invigoration by Electro Magnetism. Free on application.

25. Gr. ii., cl. 4, **1464.**

TELEGRAPH CONSTRUCTION AND MAINTENANCE CO., LIMITED,

38, *Old Broad Street, London.*

Specimens of submarine telegraph cables laid down by the company between the years 1857 and the present time.

26. Gr. i., cl. 2., **1440**—Gr. ii., cl. 4, **1458**—Gr. iii., cl. 5, **1472**—Gr. iv., cl. 6, **1485**—Gr. iv., cl. 7, **1499.**

EDWARD PATERSON,

76, *Little Britain, Aldersgate Street, London, and* 8, *Rue Martel, Paris.*

1. Electric signals for hotels, houses, mines, yachts, &c., with all accessories.

2. Telephone exchange tables, with universal commutator, indicator, and keys specially adapted for putting subscribers quickly through to the Exchange, and one another.

3. Gower bell loud-speaking telephones, as used by the British Post Office authorities, and other departments of Her Majesty's Government.

4. Magneto "bell" telephones, in ebonite or metal leather, covered cases.

5. Carbon transmitters, with or without relays, suitable for distances up to 100 miles.

6. Lightning conductors, patent copper tape conductors in continuous lengths.

7. Manganese and other batteries for electric signalling.

8. Profs. Ayrton & Perry's Portable Electric Light Current Galvanometer.—Measures directly in webers the strongest electric light current, without calculation or reference to any table; it is very "dead-beat." The accuracy of its readings can at any moment be checked by the employment of only a single cell in the following way:—the thick wire coiled round the needle, and through which the electric light current circulates, is in reality a strand or little cable composed of ten insulated wires, and which are usually joined in multiple-arc by means of the little commutator seen in the figure. But by one quarter turn of this commutator the wires are joined in series, and now any deflection is produced by *exactly one-tenth* of the current previously producing the same deflection. To check then the value, in webers, of a current producing any deflection, we have merely to ascertain the strength, in webers, of the weak current producing the same deflection when the wires are in series, or indeed any other deflection will do equally well, since the coils have such a form given to them that the strength of a current is directly proportioned to the deflection it produces.

9. Profs. Ayrton & Perry's Arc Horse Power Measurer.—Indicates the horse power expended in any electric circuit, and the accuracy of its readings may be tested by a single cell in the way described above, the instrument, like their electric light galvanometer, being wound with multiple wires, and fitted with a commutator.

10. Profs. Ayrton & Perry's Dispersion Photometer.—Indicates directly by the readings on the scale the candle power of the brighest electric light. The electric light and photometer may be close together, as the weakening of the bright light is performed, not by distance but by the employment of a very thin concave lens, the position of which is adjust-

able. Hence the measurement can be made in a small space, and does not require, as in the case of the older kind of photometers, a long dark room.

11. Profs. Ayrton & Perry's Dynamometer Coupling.—For measuring the horse power transmitted by any shaft, as for example that connecting a direct working three-cylinder engine with a dynamo electric machine.

27. **MESSRS. W. T. HENLEY,**

North Woolwich.

28. Gr. ii., cl. 4, **1454**—Gr. iii., cl. 5, **1470**—Gr. iv., cl. 6, **1482**—Gr. iv., cl. 13, **1530.**

INDIA RUBBER, GUTTA PERCHA, AND TELEGRAPH WORKS CO.,

Silvertown, London.

RAILWAY SIGNAL INSTRUMENTS.

Walker's Patent Train Describer, Receiver, and Sender. This train describer attracts attention by a single blow on a bell, which is so constructed as to strike once only for each completed signal; and at the same time to point out upon a dial the name of the coming train.

A train signal is made by simply pulling forward with the finger a small lever opposite to the name to be sent, and pressing backward a similar lever at the name last sent.

The complete apparatus includes a "Single stroke bell" and a pair of dial instruments, the "Sender" with labels for the names of each train, and small levers or handles opposite each name,—the "Receiver" with labels to correspond, and a rotating pointer, or index in the centre of each dial.

Walker's Patent Direct Action Semaphore Repeater, shewing the three positions of the signal arm.

A complete set of Walker's Electro-magnetic Telegraph Semaphores for railway block signalling.

Complete set of Preece's Single Wire Block Signalling Instruments, consisting of semaphore, switch, and plunger.

Single Stroke Direct Action Bell, with key combined.

Single Stroke Direct Action Bell.

Morse Direct Ink Writers (receivers only) with paper wheel, fitted in drawer.

Morse Pony Sounder Instruments, post office pattern.

Morse Single Current Keys, post office pattern, on teak base.

Bains Fast-speed Receiver for Automatic Working, fitted with speed regulator.

Wheatstone A.B.C. Instruments, consisting of indicator, communicator, and clockwork bell.

The Silvertown Water Level Indicator, for showing the varying height of water in reservoirs, &c.

The Silvertown Water Level Contact Maker, for above.

Hall's patent Anemometer with Telephone Receiver, for indicating audibly at surface, rate at which air is passing through underground workings of mines.

SUNDRIES.

Medical Batteries, fitted with graduated commutator, battery reverser, and interrupter.

Samples of various forms of the new Agglomerate Leclanché and other cells, suitable for telegraph, torpedo, and mining purposes, for medical use, and electric signalling generally.

A length of Brook's Underground Multiple Cable System, laid in iron pipes, with telephones connected.

Case of samples of jute-covered cables for Brook's Underground Cable System.

Case of Submarine Telegraph Cables.

Case of Carbons for Electric Lightning.

Samples of Gutta Percha covered wires.

Samples of India Rubber covered wires.

GALVANOMETERS.

Sir W. Thomson's Reflecting Astatic Galvanometer, with four coils, in square brass case; total resistance 7,000 ohms, complete with shunts, lamp and scale.

Sir W. Thomson's Reflecting Marine Galvanometer, complete with shunts, lamp and scale.

Small Reflecting Galvanometer, with oil vessel, shunts, lamp and scale, Government pattern.

Portable Astatic Galvanometer, in brass case, with jewelled centres, having a resistance of about 1,000 ohms, in sling leather case, fitted with controlling magnet.

Tangent Galvanometer, Post Office pattern, for testing, with received currents, fitted with resistance coil, shunt, and short circuiting key.

Speaking Galvanometer for submarine cable working, with lamp, condensing lens, and scale stand.

Signalling Key for above, mounted on an ebonite base, Silvertown pattern.

RESISTANCE COILS.

Large Set of Resistance Coils, dial pattern, arranged in units, tens, hundreds, and thousands, with Wheatstone Bridge attached of two tens, two hundreds, two thousands, and two ten thousand ohms.

Set of Resistance Coils, with Wheatstone Bridge, fitted with battery and galvanometer keys, Post Office pattern, in polished wood case, with lock and key.

TORPEDO APPARATUS.

A Seven-shutter Signalling and Firing Instrument for Circuit Closing and Ground Torpedoes, adapted for both testing and firing.

Torpedo Testing Table, fitted with Wheatstone's Bridge and Astatic Galvanometer, set of resistance coils with clip for platinum wire, three-coil galvanometer, key, and commutator.

Pair of Telescopic Firing Arcs, for observing and converging stations fitted with telescopic firing arm, sights with platinum contact, levelling screws, and vernier scale.

Mathieson's Circuit Closers for contact torpedoes.

Central Pavilion—East side.

29. Gr. iv., cl. 6, **1487.**

POSTAL TELEGRAPH DEPARTMENT.

MODERN APPARATUS AS USED BY THE BRITISH POST OFFICE.

[1.] DOUBLE-CURRENT SOUNDER SET.

The key in this instrument sends a current in one direction when depressed, and a current in the reverse direction when raised. These currents actuate a polarized relay, which works a local sounder.

[2.] WHEATSTONE'S AUTOMATIC SET.

Used chiefly for "press" work. Will transmit at the rate of 120 words a minute and upwards. The message is punched out on a slip of paper in the first instance.

[3.] FAST REPEATER BOARD.

Used in connection with the Wheatstone automatic apparatus for "repeating" over long lines. The repeater set is placed at a point intermediate between the transmitting and receiving stations.

[4.] FAST DUPLEX REPEATER.

This apparatus is used for repeating the signals when working Wheatstone's automatic on the duplex system, and may be also used for ordinary single working.

[5.] UMSCHALTER SWITCH.

[6.] MODERN SET OF TESTING APPARATUS.

[7.] NUMBERING MACHINE FOR NUMBERING MESSAGE FORMS.

[8.] MODERN FORM OF TRAIN SIGNALLING APPARATUS.

Invented by Preece, and used on the South Western and other railways.

[9.] APPARATUS CONNECTED WITH PNEUMATIC TUBES.

a. Carriers for pneumatic tubes.

b. Specimens of pneumatic tubes.

c. Specimens of pneumatic tube after being 25 years in use.

d. Specimen joint in pneumatic tube.

e. Double Sluice Pneumatic Valve for continuous working.

f. Single Sluice Valve for intermittent working.

g. Block Signalling Apparatus for notifying the departure or arrival of carriers.

h. Terminal Signaller. Automatic signalling apparatus for notifying the arrival of carriers at the distant station.

i. Intermediate Signaller. Automatic signalling apparatus, to be fixed intermediate upon a pneumatic tube. The duration of the electric contact can be regulated to any required length by means of the adjusting screw which limits the admission of air to the cylinder.

j. *Mandrel* used in making joints in pneumatic tubes.

k. *A Pneumatic Puncher* for Wheatstone's automatic apparatus.

[10.] WATER LEVEL INDICATOR.

For indicating the height of the water level in a reservoir. The "transmitter" sends intermittent currents in one direction or the other, according as a float in the reservoir rises or falls. The "receiver" is worked by a polarized armature, with an escapement, which is propelled in one direction or the other, according as positive or negative currents are received from the "transmitter."

Central Pavilion—West side.

Gr. iv., cl. 16, **1553.**

HISTORICAL TELEGRAPHIC APPARATUS.

Date.
[1.] 1816. RONALDS' ELECTRIC TELEGRAPH (lent by Mr. Latimer Clark).

This was a Dial Telegraph. Two dials (separated by a wire) were rotated synchronously by clockwork. When a letter which it was desired to transmit appeared at the opening in the dial at the sending end, a charge of statical electricity from a Leyden jar was sent into the wire, which caused two pith balls at both the sending and receiving ends to diverge simultaneously, and thus to indicate the letter. Words were thus spelt out. Sir Francis Ronalds used copper wire insulated in glass tubes protected by a trough of wood well tarred. A portion of this experimental line laid down in his garden in Hammersmith on the Thames in 1816, and the original model of the telegraph, is exhibited. His book, published in 1823, the first work on the electric telegraph in the English language, is kindly lent by Mr. Latimer Clark, and exhibited in another part of the exhibition.

a. Portion of the original telegraph line laid by Ronalds in 1816.

b. Original model of Ronalds' Dial Telegraph (1816).

c. "Fossil" Telegraph.

d. Insulators used by Morse.

e. Original type cast in December, 1832, by Morse.

f. Specimen of first cable between England and France, 1850.

[2.] 1837. THE "FOSSIL" TELEGRAPH.

The first underground practical telegraph laid between Euston and Camden by Cooke & Wheatstone in 1837. It was formed of copper wires covered with cotton and pitch, and laid in grooves in lengths of wood of a triangular section, the grooves being fitted up by strips of the same material. The wood so prepared was buried underground. It was used in connection with Cooke & Wheatstone's Five Needle Telegraph.

a. "Fossil" Telegraph (1837).

b. Specimen of first cable between England and France (1850).

c. Specimen of first Atlantic cable (1858).

[3.] 1837. COOKE & WHEATSTONE'S NEEDLE TELEGRAPH.

Five needles or magnets were employed in conjunction with five Schweigger's multipliers, forming five distinct galvanometers. Each multiplier was connected in the circuit of a separate wire and each needle had two movements, limited by stops, the one movement being to the *right*, and the other to the *left*. The alphabet was formed by the convergence of two needles simultaneously deflected, the letters being engraved on the dial at the points where the projections of the needles met. The currents were transmitted and reversed by depressing simple commutators. Currents from galvanic batteries were used. The five line wires were insulated as shown in the "Fossil" Telegraph. The instrument shown was used between Paddington and West Drayton in 1838. This instrument, from its peculiar shape, is known as the "Hatchment" Telegraph.

[4.] 1838. COOKE & WHEATSTONE'S FOUR-NEEDLE TELEGRAPH.

Since C, Q, J, U, and Z are very little used in the English language, it was found that the remaining letters of the alphabet could be formed by the deflection of four needles, and such an instrument was employed on the London and Blackwall Railway in 1840. In this instrument, the letters H and P are formed by one needle only.

[5.] 1842. COOKE & WHEATSTONE'S DOUBLE - NEEDLE TELEGRAPH.

Owing to the frequent failure of one or two out of the four wires, it was soon found by the operators that an alphabet could be formed by two needles only, if advantage were taken of repeated momentary movements

either separate or combined, instead of prolonged deflections. This identical instrument was fixed at Slough in 1842. Kindly lent by Messrs. Reid Bros.

[6.] 1850. COOKE & WHEATSTONE'S DOUBLE NEEDLE in a gothic case made specially for the new Houses of Parliament.

[7.] 1852. THE MODERN FORM OF DOUBLE NEEDLE modified by Messrs. Edwin & Latimer Clark, still used to a small extent on some railways in England.

[8.] 1846. THE SINGLE-NEEDLE INSTRUMENT OF COOKE & WHEATSTONE.

It was soon found that an alphabet could be formed by the movements of one needle only, by taking advantage of momentary deflections of the needle to one side or the other. At first an arbitrary alphabet was used, but in the

[9.] 1869. MODERN FORM OF SINGLE NEEDLE,

The German Unison Morse Alphabet was adapted, and the form of the instrument much improved. The change of alphabet was made in 1855.

[10.] 1843. BAIN'S I. AND V. TELEGRAPH.

The alphabet is formed by the movements of two pointers, one moving to the right and the other to the left, according to the direction of the current. The pointers are attached to circular magnets moving inside coils. A code similar in principle to that of the single-needle instrument was employed for representing the letters.

[11.] 1846. HIGHTON'S GOLD LEAF TELEGRAPH.

A small strip of gold leaf inserted in a glass tube passes through the field of a permanent magnet and forms part of the line circuit. When a current passes through the gold leaf, it moves the latter to the right or left, according to the direction of the current. The alphabet is made by repetitions and combinations of these movements, as in the Single-Needle Instrument. Batteries and commutators were used as in Cooke & Wheatstone's single-needle system.

[12.] 1848. HIGHTON'S NEEDLE TELEGRAPH.

Used by the British and Irish Magnetic Telegraph Company. A

horse-shoe or circular magnet within a circular coil, and worked by a reversing key or commutator, is used. The signals are similar in principle to those of Cooke & Wheatstone's single needle.

[13.] 1852. HIGHTON'S NEEDLE TELEGRAPH.

Small form with folding doors. Used by the British and Irish Magnetic Company. Identical in construction with the original form which is superseded.

[14.] 1848. HENLEY'S MAGNETO-ELECTRIC DOUBLE-NEEDLE INSTRUMENT.

Used by the British and Irish Magnetic Company. The needles only move on one side of their vertical position, and the signals are made up of the single and combined movements of the two needles. This instrument requires two line wires, and is worked by the magneto-electric current generated by moving the handle or handles. The interior needles are small straight bar magnets, playing between the semi-circular pole pieces of an electro magnet. The needle remains on the side on which it is left by the last current, which passes through the coils, and does not return to its vertical position by gravity, as in Cooke & Wheatstone's needle instrument.

[15.] 1848. HENLEY'S MAGNETO-ELECTRC SINGLE-NEEDLE INSTRUMENT.

Used by the British and Irish Magnetic Telegraph Company. Similar in action to the last-named instrument, but only one needle and one line wire were employed. The alphabet was produced by combinations of short and prolonged deviations of the needle, as in the Morse code.

[16.] SERIES OF COILS AND NEEDLES USED IN CONNECTION WITH THE NEEDLE SYSTEM OF COOKE & WHEATSTONE.

a. 1846. The original form, with long coils and permanent magnetic needles.

b. 1848. *Holmes' small Diamond Needle.* This was a great improvement in the original form, as it increased the speed of working considerably by diminishing the arc of vibration of the needle.

c. 1851. *Clark's Needle.*

d. 1866. *Varley's Induced Needle.*

N.B.—This prevented demagnetization by atmospheric electricity—a very frequent source of trouble.

e. 1866. *Brittan's Undemagnetizable Needle.*

f. 1869. *Spagnoletti's Induced Needle.*

N.B.—This is the modern and best known form.

[17.] 1878. NEALE'S ACOUSTIC DIAL employed on a single-needle instrument in the place of the ordinary dial. Is very similar in action to a Siemen's Relay, but the needle is held in the vertical position by a spiral spring behind the dial.

[18.] 1880. TIN PLATE SOUNDER for converting the single-needle instrument from a visual to an aural apparatus. The sounder is fixed to the dial of the ordinary instrument, the ivory pins of which are removed; the needle strikes against the curved surfaces and produces audible sounds.

[19.] 1855. BRIGHT'S BELL WITH RELAY.

Used by the British and Irish Magnetic Telegraph Company. The single-needle alphabet is produced by striking two bells of different tones, the hammers being actuated by electro-magnets worked by a relay and local battery. The relay is double-acting, and consists of two electro-magnetic bobbins placed side by side, their ends being furnished with pole pieces turning inwards. Between these pole pieces at each end of the bobbins, the ends of permanently magnetized needles, pivoted on vertical axes, play; these needles are so placed as regards their polarity that a current in one direction moves the needle which closes the local circuit of the right-hand bell, and a current in the opposite direction moves the other needle, which closes the local circuit of the left-hand bell. The signalling key used with this instrument is similar to that used with Highton's Single Needle. This instrument superseded Henley's magnetic-electric system.

[20.] 1840. COOKE & WHEATSTONE'S A B C INSTRUMENT.

A step by step instrument. The letters of the alphabet are arranged round a paper disc fixed on the axle of an escapement wheel. The letters appear at an opening in the front of the case. The escapement is similar to the "échappement-á-cheville," and is controlled by an electro-magnet. There are as many teeth in the escapement wheel as there are letters on the revolving disc; the latter moves from one letter to the other for each current sent. The train of clockwork is actuated by a main-spring.

The communicator is so arranged that a current is sent when its spoked wheel is turned through a distance equal to that dividing the letters engraved upon it. The commutator, fixed on the axle of the revolving electro-magnet, is so constructed that the magneto-electric currents are [illegible] in the same direction.

[21.] 1840. SIMILAR APPARATUS to the foregoing, but a pointer is used instead of a revolving card.

[22.] 1858. MODERN FORM OF WHEATSTONE'S A B C INSTRUMENT.

A magneto-electric instrument. The currents for working the same being generated by the revolution of an armature in front of polarized electro-magnets. The depression of a key opposite a letter arrests the motion of the pointer and cuts off the currents going out to line.

[23.] COLLECTION OF KEYS.

a. 1852. *Simple Spring Key* used with Bain's chemical recording telegraph.

b. 1854. *Key for sending* a short reversal after each signal, two sets of batteries being required. When the key is up, the line wire is connected to the receiving apparatus.

c. 1854. *Varley's Wheel Key.* A constant current is maintained on the line, and signals are made by depressing the key, and thus reversing the current. A switch is used for making the necessary alterations to the connections for sending and receiving.

d. 1870. *Stroh's Key.* Similar in general principle to Varley's key, but the contacts are made by springs, and the switch action is attained by moving the lever of the key to the right or left as required. When the lever is at the "receive" position it cannot be depressed.

[24.] COLLECTION OF RELAYS.

a. 1868. *Andrews' Relay* for Hughes' type-printing instrument, used by the United Kingdom Telegraph Company. Its peculiarity consists in the relayed currents being of equal length and independent of the length of the line current.

b. 1858. *Whitehouse's Relay.* In this relay a small permanent horseshoe magnet oscillates between the pole pieces of an electro-magnet. The adjustment is effected by the attraction of another small permanent magnet, instead of the spiral spring generally used.

c. 1856. *Varley's Horizontal Relay.* A horizontal bar of soft iron is pivoted vertically and free to move in the interior of two cylindrical bobbins. The ends of the bar which project beyond the bobbins play between the poles of horse-shoe permanent magnets fixed at each end. The relay is adjusted by moving the stops, and consequently the soft iron bar, to one side or the other.

d. 1870. *Stroh's Relay.* Two curved permanent steel magnets are fixed side by side on the opposite sides of a vertical bar, whose ends work in pivots. The poles of these magnets play between the poles of two vertical straight electro-magnets.

e. 1864. *Soft Iron Relay.*

f. 1855. *Varley's Vertical Relay.* The coil is wound on a reel of soft iron, upon each end of which a hollow "casing" or cap of the same material is fitted, almost completely encasing the coil in soft iron. The armature is crescent-shaped, and is magnetized by induction from a compound bar magnet placed behind. The crescent-shaped portion plays between the inner ends of the casings, which for that purpose do not quite meet, but leave the central portion of the coil exposed. An ordinary magnetic needle pivoted below the coil is acted upon by the latter, and serves as an indicator to call attention. The armature is held up against knife-edge bearings by two helical springs, and the adjustment is effected by varying the tension of one of them.

g. 1855. *Preece's Duplex Relay.* Worked on the *leakage* principle. The outgoing current from either station passes through one coil of the instrument and then divides between the second coil, which is connected to line and a resistance.

[25.] 1850. BAIN'S CHEMICAL TELEGRAPH, as used by the Electric Telegraph Company in place of the double needle (No. 7). The paper ribbon was prepared with yellow prussiate of potash and nitrate of ammonia. The style is of iron. The Steinheil code—dots in two parallel lines—was occasionally used, but was entirely superseded by the Morse code of dots and dashes.

[26.] 1846. MAGNETO-ELECTRIC MACHINE, commonly called the "Thunder-pump," employed for releasing the clockwork mechanism of an electric alarum or bell. Used by the Electric Telegraph Company.

[27.] 1846. ALARUM WITH CENTRIFUGAL HAMMER.

Used in connection with Cooke & Wheatstone's first needle instruments.

Moved by clockwork; driven by a mainspring; released by an electro-magnet.

[28.] SET OF JOINTS EMPLOYED ON IRON LINE WIRES.

1842. *Cooke & Wheatstone's.*

1844. *Reid's.*

1851. *Clark's "Britannia" Joint.*

[29.] 1848. WHEATSTONE'S ORIGINAL FORM OF RESISTANCE COILS FOR TESTING.

[30.] RAILWAY SIGNALLING INSTRUMENTS.

1845. *Early form of train signalling apparatus,* devised by Cooke, and used on the Norfolk Railway.

1854. *Edwin Clark's Block Signalling Instrument,* fixed on the London and North Western Railway.

1862. *Preece's Semaphore Signal,* used on the London and South Western Railway.

[31.] LIGHTNING PROTECTORS.

Selection of Lightning Protectors used at different periods.

a. Latimer Clark's Protector.

b. Henley's Protector.

c. S. A. Varley's Carbon Protector, or Lightning Bridge, with longitudinal section of the same.

d. Varley's Vacuum Tube.

e. Reid's Protector

f. Twisted Wire Protector.

g. Tube Protector, with outer glass tube.

h. Ordinary tube protector used in the Post Office Telegraph Department.

i. Comb Protector (exterior and interior).

j. Modern Plate Protector, with grooves.

k., *l.*, *m.* Old forms of Protectors.

n. Varley's Original Vacuum Protector.

o. Varley's Vacuum Protector (later form).

p. Cable Lightning Protector, in use by the Post Office Telegraph

Department, having in addition to plates a metallic bobbin, electro-nickel plated, in connection with the earth. It is wound with a fine platinum wire covered with silk and passed through melted paraffine wax, the wire being placed in the line circuit. The metallic bobbin is carefully lacquered to secure insulation in damp situations.

q. Modern Plate Protector without grooves, in use by the Post Office Telegraph Department.

[32.] INSULATORS.

A selection of insulators as used in England at different periods.

a. Iron wire. } W. F. Cooke's original goose-quill insu-
b. Three-strand copper. } lator, which was used on the line between Paddington and West Drayton in 1838.

c. Cooke & Wheatstone's original ring insulator (1840).

d. Cone insulator, original form (1845).

e. „ „ later „

f. „ „ form for repairing broken insulators.

g. Walker's double-cone insulator (brown). (1850.)

h. „ „ „ (white).

i. Physick's insulator (1847), in which the bolt by which the wire is suspended is insulated by mastic cement in the cavity at the top of the insulator.

j. Edwin Clark's brown earthenware insulator, with zinc cap (1850).

k. Do., with groove for wire.

l. Do., glass instead of earthenware.

m. (1851) brown earthenware.

n. „ glass.

o. Latimer Clark's invert insulator, corrugated (1856).

p. Varley's double-cup insulator, fitted.

q. Ditto, in separate parts, 1862.

r. Varley's No. 11 wire double-cup insulator.

s. Brown earthenware insulator called (Z).

t. Jobson's invert white, double shed.

u. „ „ single „

v. South Devon invert insulator (brown).

w. „ „ „ (white).

x. Andrews' invert with ebonite inner shed.

y. „ „ bolt tube „

z. „ „ bolt tube and shed.

A. Ebonite insulator, single shed.

B. Old form of shackle.

C. Bright's shackle.

D. Modern terminal insulator.

E. Cordeaux's screw insulator (1877).

[33.] Head of pole, showing mode of leading in and terminating.

[34.] Old form of testing tablet.

[35.] COLLECTION OF BATTERIES.

a. 1844. *Sand Battery in wooden trough.* This battery was in general use in England on the lines of the Electric Telegraph Campany from 1844 till 1854.

b. *Sand Battery in Gutta Percha Trough.*

c. 1853. *Trough form of Daniell's Battery.* This superseded the sand battery and was modified and introduced by Mr. John Fuller.

d. *Chamber Daniell Battery.* A modified form of trough battery introduced by Mr. John Muirhead in 1857.

e. Small-size ditto.

f. *Daniell's Battery.* As used by the Postal Telegraph Department.

g. *h*. *Bichromate Battery (large and medium sizes).* As used by the Post Office Telegraph Department, introduced by Mr. John Fuller, 1876.

i. *Gravity Leclanché Battery.* As used by the Post Office Telegraph Department.

1862. *Varley's Secondary Battery.* Used for some years in connection with the time signals at the central station. It was charged by an ordinary battery for about half an hour previous to giving the time signal.

[36.] SPECIMENS OF SUBMARINE CABLES IN USE BY THE P.O. TELEGRAPH DEPARTMENT.

a. Isle of Man (shore end).

b. Wexford, old (deep sea).

c. Reuter's (shore end).

d. Belfast.

e. Seven-wire, shore end. Modern type.

f. One ditto ditto.

g. Dublin (shore end).

h. Seven-wire, deep sea. Modern type.

i. Wexford (deep sea).
j. One-wire (deep sea). Modern type.
k. Firth of Tay.
l. Wexford (deep sea).
m. North Lowestoft (deep sea).
n. Wexford, old (shore end).
o. South Lowestoft (shore end).
p. Dublin (deep sea).
q. North Lowestoft (shore end).
r. Reuter's.
s. Channel Islands (shore end).
t. Queen's Ferry.
u. Channel Islands.

Against the North Wall at the North West corner of the Post Office Central Pavilion.

30. Gr. iv., cl. 6, **1478**—Gr. iv., cl. 8, **1504**—Gr. iv., cl. 13, **1529**.

E. B. BRIGHT,

45, Gerrard Street, London.

Street Fire Alarm. Adopted in London by the Metropolitan Board of Works. Consisting of a set of street posts and wall boxes fixed in position and connected by a wire, with a set of alarm apparatus for a fire brigade station.

This system of fire alarm works on the principle of the electrical balance and dispenses with all clockwork and intricate mechanism. A small electro-magnetic coil of definite resistance is place in each street post or wall box, and is brought into circuit by pulling the handle. This increases the electrical resistance of the wire along the streets, which had been previously balanced by a corresponding resistance in a rheostat at the fire brigade station, and the needle of a galvanometer relay is deflected, ringing an alarm until the commutator handle on being turned inserts an additional electrical resistance, equivalent to that of the alarm post, whence the call has emanated. The locality of the post is shown on the dial of the commutator.

If the street wire is out of order, attention is immediately called to it by the station apparatus ringing, on the electrical balance being disturbed.

Self-acting Fire Alarm for Buildings. Illustrated by a working model, heat detectors, localisers, and commutators. In applying this apparatus the small heat detecting boxes (about an inch square) are placed on the cornice or ceiling of each room protected. A bi-metallic spring in each box is expanded on any undue heat arising, and makes contact with a screw, which can be adjusted to any required degree of heat in excess of the normal temperature. A small resistance coil in each room enables the locality of the fire to be ascertained, on a commutator placed at the point to which the alarm is to be communicated, and the disturbance of the electric balance at the same time, rings one or more alarm bells, as in the street fire alarm apparatus already described.

Apparatus for Neutralizing Electricity, developed by friction during the spinning of wool, hair, silk, &c., with a diagram in illustration, already described in the Journal of the Society, No. 36, Vol. x.

Direct Acting Sounder for telegraph purposes. This telegraph instrument can be worked without any relay, by means of ten Leclanchè cells, through 10,000 B.A. units of resistance.

Combined Needle Instrument and Sounder. In this apparatus, the movements of the indicating needle, so largely used for railway purposes in England, are supplemented by distinct sounds.

Improved Printing Telegraph Instrument. Von Hœvenbergh's patent. The property of the Automatic Telegraph Company.

Against the Wall next to Mr. Bright's exhibit.

31. Gr. i., cl. 2, **1439**—Gr. i., cl. 3, **1451**—Gr. i., cl. 4, **1456**—Gr. iii., cl. 5, **1471**—Gr. iv., cl. 6, **1483**—Gr. iv., cl. 7, **1497**—Gr. iv., cl. 8, **1512**—Gr. iv., cl. 9. **1517**—Gr. iv., cl. 13, **1531.**

MESSRS. LATIMER CLARK, MUIRHEAD, & CO.,

Regency Street, Westminster.

Globe reservoir battery.

Daniells' battery, in glass jar, complete.

12-cell Daniells' battery, ordinary pattern, slate partitions.

Callaud's gravity battery, Daniells.

Muirhead's 10-cell porcelain chamber battery.

Muirhead's 10-cell ebonite chamber battery.

Siemens' sulphate battery, with paper pulp.

Marie Davie battery.

Chester battery.

Meidenger's battery.

Bichromate of potash battery.

Muirhead's platinized carbon battery.

Smee's battery.

Groove's nitric acid battery.

Bunsen's battery.

Howell's patent battery.

De la Rue's chloride of silver battery, small size.

Ditto ditto, large size.

Clamond's thermo-electric battery.

Copper and zinc plates for batteries.

Porous cells for batteries.

Perforated stoneware cells for batteries.

Carbon plates for batteries.

Battery syringe of ebonite.

Acid pourers, gutta percha.

Insulators, various.

Poles, various sizes. Muirhead's patent compound iron tubular form.

Single needle instrument, Post Office pattern, with drop handle and Spagnoletti's undemagnetizable dial.

Single needle instrument, Post Office pattern, with Highton's key and Spagnoletti's undemagnetizable dial.

Single needle instrument, Indian pattern, as used in India and the Colonies.

Single needle dial, with Spagnoletti's undemagnetizable needle.

Single needle dial, with Varley's undemagnetizable needle.

Ink recording telegraph instrument for terminal or intermediate stations.

Relay, Post Office pattern.

Relay, Siemens' pattern.

Relay, Muirhead & Winter's patent, for quadruplex working, with double tongue.

Relay, American pattern, with soft iron cores.

Sounder, Post Office pattern.

Lamp, scale stand, and lens and stand, for Thomson's mirror speaking galvanometer.

Condenser, 20 microfarads, capacity, subdivided, high insulation.

Railway signalling block instrument, Winter's patent.

Electric lock for signal point levers, Winter & Stoney's patent.

Lightning protector, Belgian pattern.

Lightning protector, Indian pattern.

Lightning protector, Maye's pattern.

Lightning protector for submarine cables, Saunders'.

Lightning protector for submarine cables, Jamieson's.

Lightning protector for submarine cable and land line connection, Post Office pattern.

Lightning protector for submarine cables, Eggington's automatic.

Smith's translation switch for submarine cable working.

Saunders' translation switch for submarine cable working.

Saunders' translation commutator, used in connection with the translation switch.

Single current transmitting key.

Highton's single needle reversing key.

Short-circuiting key, on ebonite pillars.

Lambert's charge and discharge key.

Battery reversing key, on ebonite pillars.

Graham's British Association testing key.

L. Clark's standard mercurous sulphate element, E. M. F., 1.457 volts.

Liveing's patent fire damp indicator and measurer, for use in coal mines.

Cable signalling key.

Cable signalling key, Saunders' improved pattern, with rigid arms.

Commutator board for simplex and duplex working on cables.

Short-circuiting key.

Single contact key.

AMERICAN APPARATUS.

Combination set (key, relay, and sounder).

Duplex set.

Quadruplex set.

Repeater set.

Pocket relay.

No. 1 sounder.

No. 1 key.

A 10 by 12 Western Union switch board.

Thomson's mirror astatic galvanometer, on tripod stand.

Thomson's mirror astatic galvanometer, with ebonite coil frames, to prevent cross leakage at the coils, and on hinges, to allow of easy access to the coils or needle.

Same as above, small pattern.

Battery syringe, brass.

Marine galvanometer for use on board telegraph ships.

Post Office resistance coils, as used by the Government telegraph authorities for testing telegraph lines, cables, batteries, &c.

Resistance coils, 1 to 10,000 in the aggregate (small portable set).

Medical battery, 20 cells, chloride of silver, and fittings.

Quantity galvanometer, with vertical needle and thick wire coils.

Portable galvanometer for linemen, with quantity and intensity coils.

Portable galvanometer, brass case, jewelled centres, 1000 ohms resistance.

Single needle-speaking detector, can be used either for speaking or testing purposes.

Galvanometer, with double shunts. L. Clark's pattern.

Schwendler's tangent galvanometer, with two coils, and set of shunts for comparing resistances and electro-motive forces.

Inductive resistance, Muirhead's patent.

Resistance bridge, specially designed for balancing cables on Muirhead's duplex system.

$\frac{1}{4}$ unit rheostat, used with Muirhead's patent system of duplex working.

High resistance rheostat, used with Muirhead's patent system of duplex working.

Pole changers or battery reversers for quadruplex working on Muirhead & Winter's system.

Resistance coils, 1 to 10,000 ohms, with bridge.

Resistance coils:—units, tens, hundreds, thousands, with rectangular blocks, and proportional coils.

Standard resistance coils—1 ohm.
10 ohms.
100 ohms.
1000 ohms.

Rheostat, post office pattern, for duplex working.

Thomson & Varley's slide resistance coils, registered design.

Condenser, standard $\frac{1}{3}$ microfarad capacity.

Condenser, standard ½ microfarad capacity.

Condenser, standard 1 microfarad capacity.

Condenser, standard 1 microfarad capacity, sub-divided 1, 2, 3, 4 = 1 microfarad.

One eccentric clamp.

One pair of climbers and straps.

One splicing clamp.

One splicing wrench.

One Uhlich soldering pot.

AMERICAN APPARATUS.

No. 50 hotel annunciator.

No. 6 call ditto.

No. 6 burglar alarm ditto.

Young window spring.

Lewis ditto.

Spear door ditto.

Western Electric ditto.

4-inch skeleton vibrating bell.

6-inch ditto.

4-inch ditto, lock attachment.

4-inch ditto, constant ringer.

Compound push board.

Bell pushes, various.

No. 2 electric pen outfit.

Gamewell fire alarm box.

8-inch gong.

Call box.

4-wire arm, disk, spring jack.

Union swivel screw.

Wire stay.

Stay rod, 8½ feet long.

Stay rod for iron pole.

Pole bolts.

Draw-vice and key.

Draw-tongs.

6-inch cutting pliers.

8-inch ditto.

Soldering irons.
Fletcher's patent soldering tool.
Joint-twisting tool.
Gun-metal muff for wire jointing.
Spanish spoon.
One set of contact rods.

North West corner of the Table standing at the North West of the Post Office, Central Pavilion.

32. Gr. iii, cl. 5., **1475**—Gr. iv., cl. 12, **1527.**

SIR WILLIAM THOMSON,

The University, Glasgow.

Absolute electrometer. For description see British Association Report for 1867, Committee of Standards of Electrical Resistance, or Thomson's Reprint of Papers on Electro-Statics and Magnetism. Maker, James White, 241, Sauchiehall Street, Glasgow.

Quadrant electrometer. For description see British Association Report for 1867, Committee of Standards of Electrical Resistance, or Thomson's Reprint of Papers on Electro-Statics and Magnetism. Maker, James White, 241, Sauchiehall Street, Glasgow.

Improved mariner's compass, with sun and star azimuth mirror and binnacle containing correctors for quadrantal, semi-circular, and heeling errors. See instructions which accompany the instrument. Maker, James White, 241, Sauchiehall Street, Glasgow.

33. Gr. iv., cl. 10., **1520.**

J. A. RUDGE,

1, *New Boud Street Place, Bath.*

MAGNETO-ELECTRIC FLESH RUBBER OR BRUSH.

A Siemens' armature is made to revolve between the poles of a magnetised steel frame by means of cog wheels. To the axis of the principal wheel is attached a circular rubber made of felt, or a brush of fine

silver wire. The rubber is first to be drawn through a plate containing water, and then drawn over the part where the electricity is to be applied. By this means a gentle current is obtained, which is very beneficial for medical purposes. It can be used by the patient without any assistance, it is very portable, and can be carried in the pocket, supplying a want long felt by the medical profession.

34. Gr. iv., cl. 6—Gr. iv., cl. 13.

C. E. SPAGNOLETTI, M.I.C.E.,

Great Western Railway Station, Paddington, London.

ELECTRICAL FIRE ALARM.

For giving immediate information of an outbreak of fire, at or near any given street, or place, to the depôt where the engines are kept.

This system consists of an apparatus fixed in the engine depôt, and worked from various given outlying stations, one line wire is only required, a constant current is always on the wire, so that it is always under test, and proved correct. Any accident to the wire at once causes a bell to ring, and thus reports itself defective.

In the engine house is fixed a dial with a pointer travelling round it, like the hand of a clock. The dial is divided into divisions, and each division shews the name of a street or place, a bell and galvanometer is attached, the latter is always deflected, a plunger is attached to the instrument for resetting the pointer, and which at the same time acknowledges to the station sending the alarm the receipt of its signal.

The apparatus at the outlying station is a box containing a bell, and a metal ball that rolls down an inclined plane, making contact as it runs, and sending currents which act on the dial apparatus at the engine depôt, so that in case of fire the handle has but to be lifted, and the ball at once begins to roll. This sets the bell ringing at the engine house, and the pointer to work, and it stops pointing to the street from whence the alarm was given. At the engine house the plunger is pushed in which resets the pointer of the dial instrument, and rings the bell in the box at the place the alarm was sent from. On receiving this the man sets his ball again, and this resets the galvanometer and stops the ringing of the bell at the engine house, thus giving notice that he has reset the ball ready for another signal, and that all is in perfect order again. This is under the consideration of the London authorities.

INDUCED NEEDLE FOR NEEDLE TELEGRAPH INSTRUMENTS.

About 60,000 of these are now in use. It effectually prevents a magnetic needle from the effects of lightning, getting weak, neutralized or reversed, the signals are very strong, and the beat of the needle precise and distinct.

Permanent magnets, so placed outside the coils as to induce magnetism into the inner soft iron needle, which is divided in its axle by a piece of brass to divert the magnetism to its proper place. The inner needle being of soft iron, and being induced by the outer magnets, can never be discharged, and therefore always remains the same. Delays, expense of labour, and repairs to instruments are saved, and broken pivots avoided. It is now nearly generally used on all railways, and in the Colonies, and the Government have some on trial.

ELECTRICAL LOCKING OF RAILWAY SIGNAL LEVERS.

By this system the starting signal lever is unlocked by the station in advance, before it can be pulled over to lower the semaphore arm for an engine driver to start. The operation is this: suppose A and B to be two stations and a train is waiting at (or approaching if a fast train to pass) A, the signalman rings a bell by a certain code to B, signifying "take lock off lever," B replies by returning the same code on the bell, and takes the lock off; an indicator at A shows him "lock off." The signal is lowered then by A for the train to depart, and is put to "danger" again after the train's departure to protect it from a following train, and by this act the lever is again locked; it can only be unlocked by B, and B cannot unlock it again until the train has arrived at the given point at his station required to reset the apparatus Modified arrangements of this system are made for working large yards, where shunting operations incessantly occur, for the prevention of accident and damage to stock.

The system consists of lever lock, a catch and discharge apparatus, a plunger, key or button for working, and a resetting apparatus.

It is being fitted on the Great Western Railway, and apparatus is being made for putting it on the Metropolitan Railway, London.

SPAGNOLETTI'S BLOCK TELEGRAPH INSTRUMENTS.

As used in England on the Metropolitan Railways and Great Western Railway, &c., in the Dutch, Rhenish, Holland, and in South Australia.

This instrument always shews the condition of the line, whether "clear" or blocked, train on line; it self registers the exhaustion of the battery, thereby giving a signalman three or four days' notice that the battery power requires attention, and also any defect to the line, wire, instruments, &c. It is simple in construction, not easily deranged, and very simple to work. It is adapted for three line wires, two line wires, and one line wire. It can be worked on the permanent current system or otherwise. The pressure of the red key with red table produces the red signal, "train on line," and the white key with the white table the white signal "line clear." The colours assimilate the telegraph signals with the ordinary visual signals used on a railway by day and night. A bell is used in conjunction with this instrument, by which certain codes are sent and returned, conforming the working of the instrument, which adds to safety in working a railway. This system of working, and of colouring instruments, has since become general.

35. Gr. iv., cl. 8, 1510.

KILLINGWORTH HEDGES,

25, Queen Anne's Gate, Westminster, S.W.

1. A Hedges' patent electric lamp, in which the carbons fall by gravity down two inclined troughs on to a block of refractory material, which considerably augments the brilliancy and improves the colour of the light. Samples of blocks.

2. A Hedges' patent focussing electric lamp, of simple form, in which the positive carbon is supported by means of a smaller carbon meeting it at or near the point, the negative being held up by means of a stop. The lamp will take carbons 2 feet in length, the resistance being always the same, as the current enters at the lower end contact. The separation of the carbons is effected by means of an electro-magnet.

3. A patent differential lamp, similar to the above, but separated by means of a solenoid with novel form of core and high resistance magnet, arranged to neutralize action of same.

4. Pointed iron weights, for insertion in the troughs of Hedges' lamps, for the purpose of enabling the short lengths of carbon to be used.

5. The Electric Light Supply Company's standard form of patent two-way switch, in which sparking is reduced to a minimum, and the wearing surfaces can be adjusted.

6. Deviator and electric light indicator, as designed for use in the Liverpool Docks. The deviator enables the current from a spare machine to be instantaneously switched on to any circuit where the current is found to fail. The indicator shows on which circuit this takes place and rings a bell when the current ceases.

7. Drawings and photographs of Hedges' lamps, arrangement of portable electric lighting tackle, with special frictional driving gear, and standard self-supporting beacon posts, as used in the Liverpool Docks.

36. Gr. iv., cl. 7, **1501.**

C. J. WOLLASTON,

28, *St. Petersburg Place, Bayswater. London.*

Portable naval and military loud-speaking telephones, used by the British Government Naval and Military Authorities.

37. Gr. i., cl. 2, **1437**—Gr. 1, cl. 3, **1448**—Gr. iii., cl. 5, **1468.**

COXETER & SON,

23 *and* 24, *Grafton Street East, Tottenham Court Road.*

MEDICAL BATTERIES.

Components: –Zinc;—platinum, and peroxide of manganese, &c., charged with chloride of ammonium.

The fluid capacity of each cell is fully equal to that of the Leclanché medical cell, but a battery of corresponding number of cells occupies considerably less space; they are therefore really portable, and the electromotive force and constancy are everything that can be desired.

These cells are used either for galvanic batteries or for working coils.

ACCESSORIES.

Collector.—This is so constructed that it is impossible for any portion of the battery to remain short circuited, but it allows of increase or decrease of current strength without interruption of the current.

A new plan is also introduced which allows of the cells being equally worked; this has only hitherto been accomplished by employing two dials.

Galvanometer.—This is fitted into the lid of the case and is the most sensitive vertical galvanometer hitherto constructed. When the circuit is

completed with the patient it shows by the deflection of the needle the actual amount of current strength marked approximately in Milleveber. An adjunct specially serviceable in diagnosis is a lever that is moved forward (by means of a screw head), and which acts at will as a stop to prevent the needle falling back to zero at each interruption of the current; the swinging of the needle is thus prevented and the deflection noted instantaneously.

Rheostat.—The graduation of strength in the induced current is obtained by a rheostat attached to the element beard of the coils.

The batteries are made up in various sizes and styles, the newest arrangements being in Combination batteries. The most important of these combines in one case a 60-cell galvanic battery, a coil battery, and a delicate galvano-caustic battery, all worked by the same elements, arranged so as to be worked either for quantity or intensity by a very simple contrivance.

38. LATIMER CLARK & MUIRHEAD,

24, *Regency Street, Westminster, London.*

Trophy of Telegraph Insulators in the centre of this Table.

Table due South of the last. North West corner.

39. Gr. iv., cl. 10, 1519.

J. L. PULVERMACHER,

194, *Regent Street, London, and* 63, *Rue Chabarol, Paris.*

High tension voltaic chain battery of novel construction and of instantaneous action, with restorable zinc elements.

Pocket voltaic battery, for medical use.

" " " for private use.

Electrolytic dosometre, an instrument for measuring small quantities of electricity.

Electro-Physiological chain bands for dry application.

Safety fastening electrodes.

Electrode flesh brush and hair ditto.

Ditto tooth brush.

Ditto slippers.

Ditto safety fastening pads.

Ditto tissue sheeting for diffusing high tension electricty to persons whilst lying in bed.

40. Gr. ii., cl. 4, **1465.**

THE WHITECROSS WIRE AND IRON CO., Limited,

Warrington.

Two coils of telegraph wire as supplied to the English, Indian, and Colonial Governments, manufactured solely in their patent rolling mills and finished by special appliances. Weight of pieces, 150 lbs. without weld or joint. Binding wires for telegraph use, and sample coil of special homogenous wire for submarine cables.

41. Gr. i., cl. 2, **1443**—Gr. ii., cl. 4, **1462.**

JAMES STIFF & SONS,

London Pottery, Lambeth, London.

VARIOUS POROUS CELLS AND STONEWARE BATTERY JARS.

Specially adapted to each other; the porous cells made in all sizes, with glazed upper edges.

Flat porous plates, all sizes.

Flat porous linings in red and white ware.

The stoneware insulators are thoroughly vitrified and made in all the usual shapes.

Models of condensing coils, receivers, retorts, stills, dip arms, acid pans, plumbago crucibles, and all kinds of stoneware chemical apparatus specially suited for electrical and telegraphic purposes are also shown.

42. Gr. ii., cl. 4, **1457**—Gr. vi., cl. 15, **1549.**

R. S. NEWALL & CO.,

180, *Strand, London, W.C.*

Samples of early submarine cables from 1851 to 1858.

Samples of copper cords for electric lighting.

Samples of copper rope and ribbon lightning conductors.

Samples of iron rope and ribbon lightning conductors.

Points and fastenings for above.

Anderson's portable galvanometer for testing lightning conductors.

43. Gr. iv., cl. 8, **1506**.

S. COHNE,

83, Gracechurch Street, London.

1. An electric lamp of a simple construction, based on hydrostatic principals. No mechanism, and is not easily to be put out of order, and of a very cheap cost.

2. Transparent globe, allowing the light to traverse without being hurtful to the eyes. It is based on physical law. When the light passes from one medium to another there is a partial reflection.

3. Carbons emanating a white and steady light, as the ordinary carbons in use.

44. Gr. i., cl. 3, **1445**—Gr. iii., cl. 5, **1467**—Gr. iv., cl. 6, **1476**—Gr. iv., cl. 7, **1492**.

BLAKEY, EMMOTT & CO.,

Square Road, Halifax, England.

1. Single-needle block instrument.
2. Warburton & Crossley's patent bell instrument.
3. Warburton & Crossley's patent relay.
4. Crossley's transmitter, in glass case.
5. Crossley's transmitter, with magnetic case bell and generator.
6. An 18-in. plate electrical machine.
7. Kempe's electrical speed indicator.

45.

NEWALL & CO.,

130, Strand, London, W.C.

Trophy of Lightning Conductor Stands in the centre of this Table.

Table West of the preceding.

46. Gr. i., cl. 3, **1446.**

BRITISH ELECTRIC LIGHT CO.,

Haddon Street, Regent Street, London.

Electric arc and incandescent lamps.
Various apparatus for use in electric lightning, &c.

Stand at the centre of the West End of the British General Section.

47. Gr. iii., cl. 5, **1469**—Gr. iv., cl. 5, **1479.**

MESSRS. ELLIOTT BROTHERS,

101, *St. Martin's Lane, London, W.C.*

Exhibit: Various galvanometers, resistance coils, condensers, electrometers, keys, switches, &c., for electrical purposes.

North of Messrs. Elliott's Stand.

48. Gr. i., cl. 1, **1435.**

A. APPS,

433, *Strand, London.*

Induction coil, giving a spark in air 3 feet and a half in length.

Table South West of Messrs. Elliott's Central Stand.

49. Gr. i., cl. 2, **1441**—Gr. iii., cl. 5, **1474**—Gr. iv., cl. 6, **1488**—Gr. iv., cl. 11, **1522**.

R. SABINE,

Cromont House, Hampton Wick, London,

MOVEMENT PRODUCED IN MERCURY SURFACES BY ELECTRO-CHEMICAL ACTION.

The purpose of this apparatus is to demonstrate that the currents which have been observed by moving mercury electrodes are due to different degrees of oxidation of the exposed surfaces; and that the movement of mercury globules in capillary and other tubes when traversed by currents is due to rotary motion set up through rapid oxidation and deoxidation of different points of the surface, and not to changes of capillary constant as has been erroneously supposed.

GALVANIC ELEMENT, CONSISTING OF SELENIUM PLATES IN WATER.

When a galvanic element is made up of two similar plates of crystallized selenium immersed in water, and light is allowed to fall upon one of them, the plate which is illuminated becomes electro-negative to the plate which is in the dark. The elements shown each consist of a single plate of selenium forming a separation between two water cells with lead electrodes, light being allowed to fall on one side or the other of the plate. The purpose of this construction is to lessen the resistance.

APPARATUS FOR DETERMINING THE CONTOUR OF WAVES IN TELEGRAPH CABLES.

This apparatus consists of a rotating chronoscope which acts upon two small contact keys. The first key reverses the current on starting the apparatus, and the second key, which is moved after a determined interval, removes an accumulator from a given point in the cable and discharges it through a galvanometer. In this way, by repeated experiments with different intervals, the time is found which the neutral point due to the reversal of the battery takes to travel through a given length of cable.

LANTERN SLIDE, WITH ASTATIC GALVANOMETER.

This is arranged for use with a Sciopticon. It projects the image of a straight photographed scale upon the screen, the indication being given by the shadow of an arm carried by the astatic needle.

COMMUTATOR FOR FOUR CIRCUITS.

This was constructed for experiments in which it is required to very quickly change two or four circuits from series to parellel. By the simple rotation of the barrel through a small angle, four circuits (such as galvanometer coils or elements) can in an instant be placed in series, or two and two, or all parallel.

APPARATUS FOR DETERMINING VERY SHORT INTERVALS OF TIME.

This system is based upon the known law of discharge of electrically charged bodies through resistance. It consists of an accumulator of known capacity and a coil of known resistance. The accumulator is first charged and then allowed to discharge, during the interval which is to be measured, through the resistance. The residue of charge compared with the initial charge enables the interval to be exactly calculated. Thus the time of contact of a hammer striking upon an anvil is found to be 0.00005 second.

ARRANGEMENT OF TESTING APPARATUS.

In this arrangement a discharge key is used, the three points of which are introduced into the Wheatstone Bridge system in such a way that either (1) Insulation, (2) Discharge, (3) Constants, or (4) Copper resistance can be measured with a simple set of apparatus without changing any connections and without the employment of commutators. All the connections are made in the air by thick wires which are visible.

50. Gr. iv., cl. 6, 1480.

EXCHANGE TELEGRAPH CO.,

17, *and* 18, *Cornhill, London.*

The apparatus exhibited by the Exchange Telegraph Company consists of a number of type printing telegraph instruments connected with and working from a transmitter.

The receiver contains a type wheel, which is rotated by currents in alternate directions, transmitted by means of a commutator in the transmitter working through relays. The impulses which cause rotation of the type wheel are too rapid to permit of the action of the automatic switch which controls the action of the printing mechanism, on account of the period of vibration of the automatic switch being much slower than that of the type propelment.

A pause in the transmission of the alternate currents determines the action of this part of the mechanism, and causes the printing to be effected.

Synchronism between different instruments in a circuit is obtained by means of a lever, which is carried by the motion of the type wheel into the path of a projection from the escapement axis, and eventually arrests its rotation. This detent is removed and replaced at its starting point every time a pause, which causes printing, takes place, and thus enables the transmitting operator to set the wheels at zero at any time.

The instruments are constructed by Messrs. Elliott Bros., and are the invention of Mr. F. Higgins, Engineer to the Exchange Telegraph Company, Limited, 17 and 18, Cornhill, London, E.C.

61. Gr. i., cl. 2, **1442**—Gr. iv., cl. 7, **1500**—Gr. iv., cl. 8, **1513**—Gr. iv., cl. 12, **1526**—Gr. iv., cl. 13, **1538.**

THE SCIENTIFIC TOY AND GENERAL NOVELTY CO.,

Kirkwood Road, Peckham, London, S.E.

1. *Inexpensive Toys for the instruction and amusement of youth.*

Boxes of apparatus, comprising horseshoe and bar magnets, compass, magnetic fish, &c., and book of experiments.

Boxes of apparatus, comprising Electrophorus, Leyden jar, Electroscope Discharger, insulated table, with balls, &c., and book of experiments.

A model electric lamp, automatic action, consuming "Renier" carbons, burns two hours without attention, with instructions for constructing battery for working same.

2. *Cheap Apparatus for Children and Beginners.*

Bichromate Fuller Bunsen and other batteries.

3. *The Galvanaphone, or Electric Songster.*

A small transmitter conveys vocal and other musical sounds to the galvanophone (another small instrument), which reproduces the same

greatly increased in volume and effect, thus, a song softly hummed at one end is multiplied and literally roared out at the other.

Comic Notions in Perpetual Motions.

A small ornamental cabinet which requires neither clockwork, spring, wire connections, galvanic pile, nor battery of any kind, but is complete in itself, and which causes all inanimate objects placed on the top of the cabinet to become apparently instantly endowed with life. The objects comprise comic heads, birds, figures, &c.

52. Gr. iv., cl. 8.

ROWATT & FYFE'S ELECTRIC CO., LIMITED,

52, Queen Victoria Street, London, E.C.

THE PILSEN ARC LAMP (PIETTE & KRIZIK'S PATENT), EXHIBITED BY JAMES FYFE.

This lamp effects automatically the regulation of the distance of the carbons without the aid of clockwork or other like mechanism by the use of solenoids having special cores, such solenoids and cores being applicable where, as in electric lamps, uniformity of attractive force has to be maintained throughout a considerable length of stroke. Two solenoid coils are placed end to end, with their axes in the same straight line, and with one iron core free to slide lengthwise within both, this core carrying at its end one of the carbons of the lamp, and being balanced when it is vertical, or running on rollers when it is horizontal. The one solenoid coil of low resistance is in the lamp circuit; the other of high resistance is in a branch circuit connecting the conductors to and from the lamp. The core is so shaped that so long as the two solenoids exert equal attractive forces it remains stationary, notwithstanding that it may have been previously moved lengthwise, so as to extend further into, or through the one coil than the other; and in order that it may have this property it is made with the mass of its metal reduced towards the ends.

Twenty Pilsen arc lamps will be shown at the Paris Exhibition, worked by Schuckert's dynamo machines. Each lamp estimated at what is popularly known as 2,000 candles.

JOEL'S INCANDESCENT ELECTRIC LAMP, EXHIBITED BY JAMES FYFE.

In this lamp the light is produced by the heating to incandescence of

the end of a thin carbon rod or pencil, which forms one electrode, and which is continuously fed through special and simple contact jaws against a fixed cylinder of copper forming another electrode. The length of carbon between the jaws and the copper cylinder is about ¾ inch, and from this the light emanates, chiefly, however, at that part near the fixed cylinder, where it becomes pointed by the action of the current, and therefore more intensely heated. In addition to the light produced by the incandescence of the carbon, there is also the glow or flame from the sides of the burning carbon to the copper electrode, similar to the flame of an arc light—the light thus taking an intermediate position between the purely incandescent system of Edison and the arc lamp of Serrin.

In the forms to be shown at the Paris Exhibition, the lamps are arranged for suspension, such as from the roof of an office, or a room, as well as in the form of a standard lamp resting on the floor, or on the top of an office table. In the latter case the copper electrode is vertically above the carbon. In either form the introduction of fresh carbons is a very simple and easy matter. The current for two sets of the twenty "Joel" lamps will be worked by two of Siemen's D 2 machines running at 700 revolutions per minute, generating from 20 to 30 webers.

53. Gr. ii., cl. 4, **1634**—Gr. vi., cl. 16. **555.**

SUBMARINE TELEGRAPH COMPANY,

2, *Throgmorton Avenue, London.*

Specimens of telegraph cables belonging to these companies which have been laid down between England and France and England and other parts of the continent of Europe.

Specimens of cables which have been broken by the dragging of ships' anchors or injured from other causes.

Specimens of cables whose exterior iron wires have been destroyed by oxidation.

Specimens of the experimental wire covered with gutta percha only and of the first iron-covered cables laid down by the companies between Calais and Dover and Ostend and Dover in 1851 and 1852.

54. Gr. iii., cl. 5, **1466**—Gr. iv., cl. 8, **1503**—Gr. iv., cl. 12, **1523**—Gr. iv., cl. 13, **1535**—Gr. v., cl. 14—**1536**.

W. E. AYRTON & JOHN PERRY,

68, *Sloane Street, London.*

1. PORTABLE ELECTRIC LIGHT CURRENT GALVANOMETER,

Measures directly in webers the strongest electric light currents, without calculation or reference to any table; it is very "dead-beat." The accuracy of its reading can at any moment be checked by the employment of only a single cell in the following way:—the thick wire coiled round the needle, and through which the electric light current circulates, is in reality a strand or little cable composed of ten insulated wires, and which are usually joined in multiple-arc by means of the little commutator seen in the figure. But by one quarter turn of this commutator the wires are joined in series, and now any deflection is produced by *exactly one-tenth* of the current previously producing the same deflection. To check then the value, in webers, of a current producing any deflection, we have merely to ascertain the strength, in webers, of the weak current producing the same deflection when the wires are in series, or indeed any other deflection will do equally well, since the coils have such a form given to them that the strength of a current is directly proportioned to the deflection it produces. (Maker of this and the following four exhibits, Messrs. Paterson, 76, Little Britain, London.)

2. DISPERSION PHOTOMETER

Indicates directly by the readings on the scale the candle power of the brightest electric light. The electric light and photometer may be close together, as the weakening of the bright light is performed, not by distance but by the employment of a very thin concave lens, the position of which is adjustable. Hence the measurement can be made is a small space, and does not require, as in the case of the older kind of photometers, a long dark room.

3. ARC HORSE POWER MEASURER.

Indicates the horse power expended in any electric circuit, and the accuracy of its readings may be tested by a single cell in the way described above, this instrument, like their electric light galvanometer, being wound with multiple wires, and fitted with a commutator.

4. DYNAMOMETER COUPLING.

For measuring the work transmitted directly by a shaft to any machine, as for example the work transmitted by the shaft of a direct working three-cylinder engine to a dynamo electric machine. In place of the ordinary coupling connecting the two sections of a shaft end on, is substituted this dynamometer coupling, consisting of two parts, connected together by spiral springs and keyed respectively to the two ends of the shaft. The extension of these springs measuring the couple exerted by the driving portion of the shaft on the driven is observed optically from the magnitude of the radius of the circle described by a bright bead attached to the coupling, and which by a simple system of light rigid links moves radially as the spiral springs are more or less extended.

5. MODEL OF MULTIPLE REFLECTOR.

To illustrate their method of greatly magnifying the indications of reflecting instruments, without increasing the distance between the instrument and scale, by the addition of a small fixed mirror placed extremely close to the moveable one.

In Gallery upstairs.

55. Gr. iv., cl. 6, **1484**—Gr. iv., cl. 7, **1498**.

LOUIS DE BEJAR O'LAWLOR,

11, *Wharton Road, West Kensington Park, London.*

Fire alarm in case of fire or other urgent cases with use of telephone. Only one house is in communication at the time with the central station. If several houses call at the same time, the nearest to the station will be the first to send a message *automatically*, and one after the other will send the messages in order, without confusion or delay in transmission.

Social telegraphy system. With only two wires communication is maintained with telephones in the line, and the matter commuicated is kept secret. The instruments are automatic.

In Historical Section upstairs.

56. Gr. vi., cl. 16, **1554**.

ROYAL INSTITUTION OF GREAT BRITAIN.

FARADAY'S ORIGINAL INSTRUMENTS OF RESEARCH.

1. A magnet made at the London Institution by an electric discharge from 70 square feet of charged surface. Present: Sir H. Davy, Pepys, Gordon, Bostock, and Faraday. (November 14th, 1820.)

2. Original apparatus with which Faraday obtained the magneto-electric spark.

A welded ring of soft iron, 6 inches in diameter, $\frac{7}{8}$ths of an inch thick, one part covered by a helix A, containing about 70 feet of insulated copper wire, occupying about 9 inches in length upon the ring.

The other part covered by a second helix B, containing about 60 feet of insulated copper wire. The helixes are separated from each other at their extremities by half an inch of the uncovered iron.

The iron ring was converted into a magnet by passing a voltaic current through the helix A. This induced an electric current in the helix B, and

a small spark was for a moment seen at the carbon terminals. (Phil. Trans. 1831.)

3. Faraday's original apparatus for magneto-electric induction by a permanet magnet.

A pasteboard tube is surrounded by a helix C of insulated copper wire. The diameter of the tube allows a cylindrical bar magnet to pass freely into it. The terminal wires of the helix are connected with a galvanometer.

On the introduction of a permanent bar magnet into the helix, and on its withdrawal from it, currents of electricity were induced in the helix, which caused a deflection of the galvanometer needle. (Phil. Trans. 1831.)

4. Various helixes, spirals, &c., used by Faraday in his researches on magneto-electric induction, &c. (Phil. Trans. 1831.)

5. Apparatus used by Faraday for the condensation and liquifaction of gases, consisting of condensing pump and connections, conducting and other tubes, gauges, sealed tubes for containing the liquified gases, &c., &c. (Phil. Trans. 1845.)

6. Bars of borate of lead glass made and used by Faraday for the action of magnets on polarized light. (Phil. Trans. 1845.)

7. Glass tubes prepared and used by Faraday for testing the magnetic and diamagnetic character of gases.

The tubes containing the gas to be examined were suspended in the magnetic field of a powerful magnet, the result being either attraction or repulsion of the tubes as the gases they contained were either magnetic or diamagnetic. (Phil. Trans. 1850.)

8. Faraday's rotating rectangle for illustrating the inductive action of the earth.

The apparatus, provided with a commutator for collecting the currents, was attached to a galvanometer, and rotated in the line of the magnetic meridian, when the electric current induced in the rectangle deflected the galvanometer needle. (Phil. Trans. 1852.)

57. Gr. iv., cl. 16, **1552.**

KING'S COLLEGE, LONDON.

[1.] ORIGINAL RHEOSTAT.

In the early experiments (prior to 1840), when resistance was required to be introduced into electric circuits, this appears to have been done by wires suspended from insulating supports (*e. g.*, Ronalds in 1820. :

Hammersmith and Wheatstone in 1834, in his "Duration of Electric Light" experiments). As galvanic electricity requires less provision for insulation, resistances (about 1840) assumed a less cumbrous form, the wire being coiled upon a frame, or upon a cylindrical block of wood, a provision sometimes being made by which the whole or only half the length of wire could at pleasure be taken into the circuit. Faraday advanced upon this by furnishing a fixed wooden cylinder, having a wire coiled helically upon it, with a sliding contact, so that any number of whole convolutions could be introduced into the circuit (*vide* Exhibit, No. 3 Rheostat given by Faraday to C. W.). In all these arrangements, the wire when not of constant length, was increased by constant augmentations, while the exact measurement was done by the galvanometer. Wheatstone's improvement consisted in making the resistance so finely adjustable that it became itself the measuring instrument, the galvanometer becoming practically only an indicator. Wheatstone's rheostat consisted of a length of fine metal wire wound partly upon each of two parallel cylinders, one of wood, with a helical groove cut upon it, the other brass, mounted upon a suitable stand. By means of a handle, these cylinders could be turned so that the wire could be wound almost entirely upon either of them. The current enters the apparatus where the wire is fixed to the metal axis of the wooden cylinder, and as the wood is insulating, the current passes through the entire length of wire upon the cylinder, and enters the brass barrel which is not insulating, at the point where it first touches it. Therefore the greatest available resistance is obtained when the full length of wire is wound upon the wooden cylinder, and the least when it is wound upon the brass. The intermediate values are exactly indicated by a scale shewing the number of convolutions unwound and by a pointer and gratuated circle at one end which shews the fractions of convolutions. The instrument (Exhibit No. 1) was shown to Jacobi, in August, 1840, and was referred to by him at the B.A. Meeting, in Glasgow that year.

[2.] NEEDLE FOR SHEWING CONCENTRICITY, &c.

[3.] RHEOSTAT GIVEN BY FARADAY TO WHEATSTONE. (See remarks of No. 1 Exhibit above.

[4.] WHEATSTONE ORIGINAL BRIDGE.
[5 and 6]. TWO PLAINER BRIDGES.

This apparatus was designed by Wheatstone (1840-3), and called by him the "*differential resistance measurer.*" The arrangement of four

circuits in which the balance principle is the same as that on which Wheatstone based his instrument, had been described for another purpose in 1833 by Mr. Christie. The bridge consists of a mahogany board, on which four copper wires are stretched between suitable binding screws. Two binding screws are on one side of the board for receiving the ends of the galvanometer, and two similar binding screws on the other side are for the battery. The four " bridge " wires are so arranged that one of them goes from *each* of the galvanometer binding screws to *each* of the battery binding screws. " If the four wires be of equal length and thickness and of the same material, perfect equilibrium is established, so that a rheomoter however powerful will not produce the least deviation of the needle of the galvanometer from zero. . . . . . . But if a resistance be interposed in either of the four sides, the equilibrium of the galvanometer will be disturbed." (Wheatstone Phil. Trans. v. 133, p. 324). A different arrangement of wires was published by Wheatstone at the same time. In this the four wires were placed upon the board in the shape of a lozenge, the extremities being connected to binding screws. The binding screws terminating the longer axis of the lozenge received the galvanometer, while those at the two ends of the shorter axis received the battery. An adjustment was introduced by means of a moveable arm attached to one of the terminals, by which a portion of the current of one of the four wires could be shunted to obtain equilibrum.

AUTOMATIC SYSTEM.

[7*a*.] *Original puncher, with ivory keys* (1858)		Puncher.
[7*b*.] *Puncher of later date*	(*ca.* 1863)	
[23.] *Ink dot printer* (1858)		Receiver.
[24.] *Printer, with metallic ribbon*		
[25.] *Six magnet dot printer*		
[58.] *Rapid dot and dash transmitter.*		Transmitter.
[33.] *Original D C key*	(1860)	Key.
[34.] *D C key for quick reversals*		

Wheatstone's automatic system, as its name implies, was designed with a view to replace manual transmission by mechanism, for the purpose of obtaining greater accuracy and increased speed. " Long strips of paper are perforated by a machine for the purpose (called a *puncher*), with

apertures grouped to represent the letters of the alphabet and other signs. A strip thus prepared is placed in an instrument associated with the source of electric power (called the *transmitter*), which on being set in motion moves it along and causes it to act on two pins in such a manner that when one of them is elevated the current is transmitted to the telegraph circuit in one direction, and when the other is elevated it is transmitted in the reverse direction. The elevating and depressing of these pins are governed by the apertures and intervening intervals in the paper. These currents following each other act upon a writing instrument (called the *receiver*) at the distant station in such a manner as to produce corresponding marks on a slip of paper moved by appropriate mechanism." (*Comptes Rendus*, 1859, v. xiviii., p. 214).

A B C INSTRUMENTS.

[8 *to* 20.] *A B C instruments.* (1840 *to* 1860.)

[26.] *A B C receiver with revolving dial.* (*ca.* 1843.)

[28.] *Combined needle and revolving dial receiver.*

[29.] *A B C receiver in black case* (1870.)

[27.] *Call bell.*

Dial telegraphs were patented by Cooke & Wheatstone in 1840, and constructed to work both with galvanic and magneto-electric currents. The use of galvanic currents, however, in this system did not long survive its invention, the form with magneto-electric currents being that almost exclusively employed. Since 1840 the system has, at various dates, been greatly improved upon, and very extensively employed both for public and private uses. The apparatus consists essentially of three distinct parts:—(1) the *communicator;* (2) the *indicator*, and (3) the *call bell.* The communicator consists of a small box upon the upper side of which is a dial having its circumference, divided into equal spaces, marked with the letters the alphabet and numerals. A hand or pointer pivotted in the centre of the dial is made to rotate, stopping at any letter at the will of the operator. In the interior is a permanent magnet and coil which produce an alternate current for every letter which the hand of the communicator passes over. The indicator has a dial in front similar in its arrangements to that of the communicator but smaller. An index hand is moved round with a step-by-step motion propelled by an electro-magnetic in its interior, its movements being controlled by and corresponding with the currents sent by the communicator. The bell is actuated by a train of clockwork released by the currents of the communicator. In 1879

there were between 7,000 and 8,000 of the improved form of these instruments in use.

[21.] *Telegraphic thermometer*

This apparatus consists of two distinct instruments connected together by telegraph wires. The first (A) was called by Wheatstone the *questioner;* the second (B) the *responder*. The "questioner," which is always retained in the operating room, consists of a rectangular wooden box having on the top a dial-face engraved with thermometer degrees. A handle in front causes the rotation of a magnetic armature in the interior, and produces alternate currents in suitable wire coils, similar in construction to the communicator of the A B C telegraph. But besides this the box contains also a small electro-magnet, the armature of which acts by means of mechanism upon the index or hand, which it propels round the engraved scale. The "responder," which is located at the distance place whose temperature is to be be measured, is contained in a circular brass box showing at the top a similar dial with thermometic degrees and an index. It contains also (1) a "Breguet" thermometer, (2) a small electro-magnet, which propels a disc, making as many steps as there are half degrees upon the scale, and (3) an axis, to which is fixed a delicate spiral spring, which causes a pin to bear lightly against the hand of the thermometer however it may vary its position. These two instruments are connected by telegraph wires. The action of the apparatus is as follows:—The dial of A being brought to O°, and the handle turned, the circuit is completed with the electro-magnet of B. A disc is thereby caused to revolve in an opposite direction to the graduation of the scale until a pin, originally starting from O°, comes into contact with a stop in the thermometer index, and completes the circuit of a second wire, so that now both electro-magnets act simultaneously, and the index of A marces over a space corresponding with that between O° and the deflected hand of the thermometer.

[22.] ELECTRIC FIRE ALARM.

This consists of a mercury thermometer tube in which the mercury expanding by heat made contact with a platinum wire and closed the circuit of an electric bell. It was made about 1838, and was used in the inventor's house.

[30.] ELECTRO-MAGNETIC CHRONOSCOPE.

This instrument was devised by Wheatstone, in 1840, for the exact

measurement of small intervals of time principally for determining the velocity of projectiles. Two screens each made of wire led backwards and forwards across a frame are placed in the path of the ball at a known distance apart. The wire of each screen forms part of a separate circuit communicating with the indicating instrument. When the wires are successively broken by the projectile the successive interruptions of the two currents are registered by the indicating instrument, part of which moves with a known velocity.

[39.] ROTATING MIRROR, SPARK DISC, &c.

This apparatus was constructed with the object of measuring "the velocity of electricity," and "the duration of the electric light." The plan of measuring by the rotating mirror was announced by Wheatstone in 1830, but his complete experiments were not published until 1834. His plan consists essentially in viewing the image of the spark reflected from a plane mirror, which is rotated 800 times a second, and in which the reflected image of a luminous point therefore passes over an arc of half a degree in about the $\frac{1}{152000}$ of a second. In the experiments on the velocity of electricity a leyden jar was discharged through a copper wire half a mile long, interrupted both at the middle and at its two extremities. The wires connected with the coating of the jar and the ends of the copper line wire were connected with six small brass balls placed equidistant in a line with each other, so that each discharge gave three separate sparks close together. When these sparks were viewed in the rotating mirror a deviation of half a degree between any two neighbouring images would indicate a speed of 288,000 miles per second through the corresponding half of the wire. Wheatstone found that when the velocity of the mirror exceeded a certain limit the three sparks were elongated into three parallel lines and the lengths became greater as the velocity of the mirror was increased. The greatest elongation observed was about 24°, indicating a duration of the spark of about $\frac{1}{24000}$ second, whilst the spark due to the middle interruption of the line wire was never more than ½ degree in arrear of the sparks due to the two ends which were always simultaneous.

[41.] FIVE-NEEDLE DIAL AND KEY.

This form of telegraph instrument was the subject of Messrs. Cooke & Wheatstone's first patent (1837). The receiver consists of five vertical

galvanoscopes placed in a horizontal line, across the middle of a lozenge-shaped board. The needles of the galvanometers have each two limited motions, one to the right, the other to the left, determined by the direction of the current producing the deflections. Letters of the alphabet are indicated by the simultaneous deflections of two needles; but numerals by the deflection of only one. Twenty letters of the alphabet are marked upon the dial and occupy the points of intersection of the various converging lines indicated by two deflected needles; G. J. Q. U. X. Z. being omitted. The numerals are marked upon the margin of the lower half of the lozenge-shaped board. The key used with this system has five brass bars corresponding with the five galvanometer circuits, over each of which are two spring contacts, one connected with the positive, the other with the negative pole of the battery, so that by pressing the suitable button, a right or left hand deflection may be produced.

[31.] ORIGINAL RELAY.

[32.] RELAY IN CASE.

This relay was patented by Cooke & Wheatstone in 1837, and was employed for sounding an alarm in connection with their five-needle telegraph. It consists of a vertical galvanoscope coil and a magnet needle, the deflection of which causes a small metallic fork to dip into two cups of mercury and to close the circuit of a local battery and alarm.

[51.] ORIGINAL DYNAMO-ELECTRIC MACHINE.

Prior to 1866, magneto-electric machines were actuated either by a permanent magnet, or by an electro-magnet deriving its power from a battery placed in the circuit of its coil. Early in 1866, the discovery was made by Wheatstone that an electro-magnet, if it possess at the commencement the slightest polarity, may become a powerful magnet by gradually augmenting currents which itself originates. The apparatus exhibited was made for Wheatstone by Mr. A. Stroh in the summer of 1866, and was shown at the Royal Society in February of the following year. It consists of a flat electro-magnet, between the poles of which rotates a cylindrical armature of soft iron coiled longitudinally with wire, and commonly known as the " Siemens' Armature." It was found that, when the ends of the armature wire were connected in circuit with the wire of the electro-magnet, and the armature rotated, currents of considerable strength were quickly generated in the system. The explanation of this which Wheatstone gave is as follows :—" The electro-magnet always

retains a slight residual magnetism, and is therefore in the condition of a weak permanent magnet; the motion of the armature occasions feeble currents in alternate directions in the coils, which after being reduced to the same direction pass into the coils of the electro-magnet in such a manner as to increase the magnetism of the iron core; the magnet having thus received an accession of strength, produces in its turn more energetic currents in the coil of the armature, and these alternate actions continue until a maximum is attained, depending upon the rapidity of the motion and the capacity of the electro-magnet." The idea of a dynamo-electric system seems to have occurred to several experimenters about the same time. In December, 1866, Mr. S. A. Varley provisionally patented a machine of the kind; and in January, 1867, Dr. Siemens read a paper before the Academy of Sciences upon the same subject.

[54.] WHEATSTONE'S ORIGINAL DRAWINGS OF CABLE MAKING AND LAYING.

The scheme embodied in these drawings and the experiments in connection with the subject form the veritable starting point of submarine telegraphy. In 1837, Wheatstone commenced working on the subject of submarine telegraphy, and in February, 1840, he gave evidence before a Select Committee of the House of Commons on the practicability of establishing communication by means of a cable between Dover and Calais. These two drawings were prepared in 1840, in which year he visited the Continent with a view to get his suggestion carried into effect. Sheet I. shows the machines for insulating and making up the proposed cable, and how it was to be put on board the cable laying ship. Sheet II. shows the proposed route of the cable from the South Foreland to Cape Grisnez, and also the method of laying, joining, and underrunning.

A preliminary experiment, which was made by Wheatstone in 1844, in Swansea Bay, was intended to test the practicability of this scheme. He tried various forms of insulation, and telegraphed from a boat in the bay to the Mumble Head Lighthouse. The results were highly encouraging and Wheatstone was preparing seriously to carry out his scheme when gutta-percha came to his knowledge (in 1845), and promised obvious advantages as an insulator, but he failed to devise a means of applying it to the wire. A method to do this was soon afterwards invented, which enabled the line to be laid in 1849 by Mr. Brett over the identical route previously contemplated by Wheatstone.

In Bibliographical Section upstairs.

57. Gr. vi., cl. 15, **1550**.

THE SOCIETY OF TELEGRAPH ENGINEERS AND OF ELECTRICIANS,

4, Broad Sanctuary, Westminster, London.

[1.] A complete set of the Journal of the Society, 9 Vols., 1872-81.

[2.] The Ronalds Catalogue. This catalogue contains between 13,000 and 14,000 entries, comprising not only the Books, Pamphlets, and Publications in the Ronalds Library, of which it forms the catalogue, but also of all other works relating to Electricity, Magnetism, and the Electric Telegraph, which its compiler was able to discover during a long life devoted to the work.

58. Gr. vi., cl. 15, **1547**.

LATIMER CLARK, Mem. Inst. C.E.,

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Abercorn (Earl of).

Calculations and Tables relating to the attractive virtue of Loadstones. 8vo. 1729

(Interesting as giving the value of loadstones in the last century.)

Affaytatus (Fortunius).

Physicæ ac Astronomicæ considerationes. Venetis. 1549

(This early writer on magnetism was the first to publish the declination of the magnetic needle. The credit of this has been usually ascribed to Robert Norman, in his "Newe Attractive, 1581." George Hartmann had, however, observed the phenomenon at a still earlier date. See Dove's Repertorium, Vol. ii., p. 129. This work is extremely rare.)

Barlowe (Wm.)

The Navigator's supply. Sm. 4to. London. 1597

(This very scarce old tract on navigation commences with a full description of the compass.)

Magnetical advertisements. London. 1616

(A rare and early treatise on the magnet.)

Blondus (M. A.)

De Ventis et Navigatione. 4to. Venetis. 1546

(An early tract on navigation. Contains an engraving of a mariner's compass, called "Pixis vel buxolus," from which the French word "boussole" and the Italian "bussola.")

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(The earliest English work on electricity.)

FREDERICI (J. B.)

Cryptographia. 4to. Hamburg. 1685

(Contains, at page 234, the earliest example of the "Morse" code.)

GILBERT (DR. WM.)

De Magnete. Fscap. 4to. London. 1600

(Gilbert was a great chemist and physicist, and physician to Queen Elizabeth. This author is generally regarded as the founder of the sciences of magnetism and electricity.)

GREEN (G.)

Mathematical Analysis to the Theories of Electricity and Magnetism. 4to. Nottingham. 1828

(A fine mathematical treatise on electricity. The author was the first to introduce the word "potential" into physics, see page 9. The work is extremely rare.)

GUERICKE (OTTO DE).

Experimenta Nova. De vacuo spatio. fol. Amstelodami. 1672

(Guericke was the inventor of the air pump, and the plate of his experiments with this machine, at page 104, is very curious. At page 148 he gives a drawing of the earliest form of the electrical machine.)

KIRCHER (A.)

Ars Magnesia. 4to. Herbipoli. 1631

(A rare and early treatise on magnetism.)

Magnes Sive de Arte Magnetica. 4to. Rome. 1641

(An early and important treatise on magnetism.)

MARTYN (JOHN) AND CHAMBERS (EPHRAIM).

The Philosophical History and Memoirs of the Royal Academy of Science at Paris; or Abridgement of all the Papers published relating to Natural Philosophy from 1699 to 1720. 8vo., 4 vols. 1742

(At page 187, mentions experiments made by Du Vernay in 1700 before the Royal Academy of Sciences at Paris, showing galvanic convulsions of a frog's leg.)

MILNER (THOS.)

Experiments and Observations in Electricity. 8vo. London. 1783

(A very rare treatise on electricity. It contains, at page 35, a de-

scription of the kind of electrometer known as Peltier's electrometer.)

MORTENSSON (JAHANNES).

De Electricitate, sub præsidio Samuelis Klingenstierna. Sm. 4to. Upsalia. 1740

(Believed to be the first separately printed treatise on electricity.)

NECKAM (ALEX.)

De Naturis Rerum. 8vo. Roy. 8vo. London. 1863

(Alexander Neckam, Monk of St. Albans born 1157, died 1217, is probably the earliest writer who alludes to the mariner's compass, see page 183 and preface xxxv.)

NEWTON (SIR ISAAC).

Autograph Letter of. 4to. London. 1716

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NORMAN (ROBERT).

The Newe Attractive. (1st edition), 8vo. London. 1581

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OHM (G. S.)

Die Galvanische Kette. 8vo. Berlin. 1827

(The original treatise on the well-known "Ohm's Law.")

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PORTA (J. B.)

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(In twenty books. The seventh book—" De Mirabilia Magnetis," contains, at page 289, the earliest description of a supposed magnetic dial telegraph. There are numerous editions of this work. The first was published at Naples in 1589.)

PEREGRINUS (PETRUS).

De Magnete, &c. Sm. 4to. Augsburgi. 1558

(The earliest known work treating specially on magnetism—probably the rarest.)

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(A scarce and early tract on magnetism.)

RIESS (PHILIPP).

Uber Telephonie durch den galvanischen Strom. 8vo. 1861

(The first description of an electrical telephone)

RONALDS (FRANCIS.)

Description of an Electrical Telegraph. 8vo. London. 1823

(This very remarkable work describes, at page 19, the electric telegraph in nearly the same form as it exists at the present day. At page 12, he clearly forsees the effect of induction on telegraph wires.)

SCOTS MAGAZINE. (February 1, 1753.) 8vo. Edinburgh. 1753

(See page 73. The letter signed C. M., (*i.e.* Charles Marshall,) contains the earliest known mention and description of an electrical telegraph.)

SOEMMERING (S. F.)

Uber Linen Electrischen Telegraphen. 4to. Munchen. 1811

(The first description of Soemmering's experiments on the telegraph.)

STEINHEIL (C. A.)

Uber Telegraphie. 8vo. Munchen. 1838

(The first description of Steinheil's telegraph.)

STRADÆ (F.)

Prolusiones Academicæ 8vo. Lugduni. 1617

(Stradæ's curious verses on an imaginary magnetic telegraph at page 306 are well known.)

SWAMMERDAM (J.)

Biblia Naturae. 2 vols., fol. Leyden. 1737-8

(2nd vol. describes at page 839 experiments on the galvanic effect of metals on the legs of the frog, made before the Grand Duke of Tuscany.)

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(1st vol. contains his very remarkable investigations on magnetism.)

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De Natura Magnetis et Ejus Effectibus. 4to. Colonia. 1562

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Speculum Naturale. 2 vols. Fol. 1473-1476

(Contains probably the earliest printed allusion to the polarity of the magnetised needle and its use for the purposes of navigation. See vol. i., lib. ix., cap. xi. and xl. Vincentius was Bishop of Beauvais, and wrote about 1250.)

VOLTA (A).

De vi attractiva. 1769

(Volta's earliest work.)

ON THE SYMPATHETIC TELEGRAPH.

The following books contain early allusions to an imaginary magnetic telegraph, formed by two compasses, with the letters of the alphabet written round the circumference, and with the two needles magnetised by the same magnet. These were supposed when at a distance from each other to move in sympathy. This imaginary telegraph was first mentioned by Baptista Porta in 1558, who is supposed to have derived it from Cardinal Bembo. The idea has been continually alluded to or reproduced in a variety of forms by later writers down to a recent period. Among these Famianus Strada in his Prolusiones, 1617, and Addison in the Spectator 1711, are the most widely known.

1558. Porta (J. B.)

Magiæ Naturalis, &c. Libri iv. 8vo. Neapoli. 1558

(See page 90.)

1589. Porta (J. B.)

Magiæ Naturalis. Libri xx. 8vo. Naples. 1607

(1st edition 1589. At page 289, Porta gives the first clear description of the sympathetic compasses.)

1600. De Sunde (J. H.) (*i.e.* Schwenter Daniel.)

Steganologia and Steganographia. 12mo. Nurnberg. 1600

(Without date. See page 127. Janus Hercules de Sunde is an assumed name. Hiller in the preface to his Mysterum Artis Steganographicæ 1682, says that it is a synonym for Daniel Schwenter Noribergense. And at page 287 he says, "Schwenteri is Est Hercules de Sunde." De Sunde gives a more interesting description of the magnetic telegraph than any other writer. He calls the attention of his correspondent by ringing bells by means of bar magnets. His needles are also moved by bar magnets, and the letters are formed by one, two, or three strokes to the right or left as in Cooke & Wheatstone's system. His ideas are purely cabalistic, but his curious anticipations of the modern telegraph are very singular. The date is taken from the British Museum Catalogue.)

1609. De Boot (B.)

Le Parfaict Ioaillier ou Histoire des Pierreries. Lyon. 1644

(1st edition published in 1609. Translated by Andre Toll into Latin and French. See page 598.)

1609. De Boot (B.)

Gemmarum et Lapidum Historia. Lugduni Bat. 1647

(Latin translation of the above work by A. Toll. See page 464.)

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(His verses on the imaginary lover's telegraph are well known. See page 306.)

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Recreation Mathematique Composer de Plusieurs Problemes. 8vo. Paris. 1626

(1st edition published at Pont a Mousson, 1624. This work has gone through many editions and has been translated into English and Dutch. See Van Westen, 1663. See page 94.)

1629. CABEUS (NICOLAS).

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(Cabeus is the first to give a drawing of the telegraph. See page 302.)

1629. PANCIROLLUS (S.)

Rerum Memorabilium sive deperditarum. 4to. Francofurti. 1629

(See page 232.)

1630. HAKEWILL (GEORGE).

An apologie or declaration of the power and providence of God in the government of the world. Fcap, fol. London. 1630

(See p. 285.)

1631. KIRCHER (A.)

Ars Magnesia. Herbipoli. 1631

(See pages 35 and 36.)

1635. GALILEO (G.)

Dialogus de systemate mundi. Augustiæ Treboc. 1635

(1st edition 1632. See page 88.)

1636. SCHWENTER (DANIEL).

Deliciæ Physico-Mathematicæ. 3 vols. Nurnberg. 1636

(See Vol. i., page 346.)

1637. SERVIUS (PETRUS.)

De Natura Artisque Miraculis.

(See p.p. 456 and 336 of Endter (J. A.) Theatricum Sympatheticum Auctum. Norimbergae 662.)

1641. KIRCHER (A.)

Magnes sive de Arte Magnetica. Sm. 4to. Rome. 1641

(See page 382.)

1646. BROWN (THOS.)

Pseudodoxia Epidemica or Enquiries into very many received tenents, and commonly presumed truths. 4to. London. 1646

(See page 76.)

1657. TURNER (ROBT.)

Ars Notoria. The Notary Art of Solomon, showing the cabalistical key of magical operations, &c. London. 1657

(See page 136. The first English writer who gives a figure of the magnetic dial.)

1661. GLANVILL (J.)

The vanity of dogmatizing, and an apology for philosophy. London. 1661

(See page 202.)

1663. WESTEN (WYNANT VON).

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(See pages 258-280. His description is copied from De Sunde.)

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Magisterium Naturæ et Artis. Opus Physico-Mathematicum. 3 vols. Brixiæ. 1684-96

(See vol. iii., page 412.)

1690. MARANO (J. P.) or Turkish Spy.

Letters writ by a Turkish Spy. 9 vols., 22nd edition. London. 1734

(See vol. i., p. 115.)

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(See page 147.)

1696. VALLEMONT (DE).

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(The Paris edition, 1696, has an appendix, which at page 32 gives an account of the sympathetic telegraph.)

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(See the word "Lodestone.")

1762. DIDEROT.

Memoirs. Correspondance et ouvrages inédité de Diderot. Paris. 1841

(See page 278. Diderot, in his letter to Madame Volland of 28th July, 1762, alludes to Comus (Ledru) and his supposed telegraph.)

1762. DU PETIT ALBERT.

Secrets Merveilleux de la Magie Naturelle et Cabalistique. Lyon. 1762

(See page 228.)

1772. BARTHELEMY (L'ABBE).

(See correspondance inédité du Madam du Deffand. Vol. ii., p. 99.)

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CASSELL, PETTER, & GALPIN,

La Belle Sauvage Yard, London, E.C.

"Science for All," volumes ii. and iii., and monthly parts 37-41.

60. Gr. vi., cl. 15, **1546.**

W, T. EMMOTT,

41, *Tavistock Street, London.*

Bound volumes of "Design and Work" and "Mechanical World;" also copy of each week's issue published during the exhibition.

61. Gr. vi., cl. 15, **1548.**

MACMILLAN & CO.,

29 *and* 30, *Bedford Street, London.*

Books on Electricity.

62. Gr. iv., cl. 15, **1549.**

R. S. NEWALL,

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Books and Pamphlets.

Index to the Exhibits in the British Section.

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